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THE DIAGNOSIS, PATHOLOGY, AND TREATMENT OF TYPHUS FEVER.

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No. II.

General Symptoms.

The most frequent of the general symptoms of typhus fever are a sense of coldness along the vertebræ, a sense of pain or weariness in the loins and limbs, with pain in the head, and stupor. A sense of weariness, or of muscular prostration, although alone not diagnostic of the disease, yet affords us an excellent criterion for our prognosis in different stages of the fever, as it is constantly present, and is the first symptom that commands the attention of the practitioner. In proportion as the prostration is excessive, so our prognosis becomes more unfavourable. In the early stages the patient feels merely a disinclination to move; at a later period the disinclination is accompanied by an inability to maintain the erect position; and subsequently he is always recumbent. Whilst lying, however, his knees will be observed to be more or less flexed, and the head turned to one or the other side, and usually away from the light. Now, in this stage, the patient continues conscious, and still feels a sense of muscular fatigue, to obviate which his limbs assume that position in which he is least sensible of muscular action—the state of harmony of the general muscular forces. The flexed position into which our limbs naturally throw themselves during sleep is that in which the contractile force of each muscular fibre is most accurately balanced by that of its opponent, without producing consciousness, and, consequently, when the collective forces are at their minimum; for as soon as this harmony is disturbed we become sensible of the effort necessary to communicate an increase of force: this state is commonly called that of relaxation, a vague, and, therefore, a false term. While one set of muscles may be considered to be, as regards volition, in an active, another set may be also considered to be in a passive state, but not relaxed, for this term is more correctly appropriated to that state of prostration, accompanied by unconsciousness, indicative of the last stage of fever. In the passive state each muscular fibre exerts only that degree of contractility that may be called the tone of the muscle, and which, in union with the action of the whole muscular system, is merely sufficient to support the weight of the bones and soft parts. These are the resistance to the tone of the muscle. During life the lower portion of each limb acts as a weight by which the upper portion is drawn into a direct line with it; thus, the whole leg acts upon the hip-joint, the foreleg upon the knee-joint; and when the patient is lying on his back, if the muscular tone be deficient, the state of complete supination must inevitably ensue. When, however, the muscular tone is not greatly diminished, the patient will lie on his side, with flexed limbs, which is the state of rest, but not of relaxation.

When a patient is lying on his side nothing more is necessary to keep him in that position but the tone of the muscles; when this fails the legs become extended, and assist in throwing the patient upon his broadest surface—the back. Now, this is the posture of general relaxation, when the body obeys the physical law of gravity alone. The practitioner will now clearly see that when muscular relaxation occurs death must be near at hand. The conditions of the muscular system may, therefore, be conveniently divided into four stages; 1st, the stage of defective volition; 2nd, that of powerlessness; 3rd, the passive stage; 4th, the stage of relaxation.

The cause of this state of the muscular system is to be traced, primarily, to the change of conditions in the extreme texture, rather than to any influence of the nervous centres, by which the muscles are rendered incapable of answering to their natural stimulus; for it will occasionally happen, in the early stages, that there will be very trivial affection of the nervous centres when there is considerable muscular depression; in fact, there is a period when these two phe-

nomena do not bear a concurrent relation to each other. The affection of the head and spinal cord may be greater, and the muscular powerlessness less, and the contrary; therefore muscular powerlessness depends upon a peculiar change going on in the substance of the muscle itself. This position may be substantiated by an appeal to the conditions present during an attack of muco-enteritis, a disease very similar to typhus. Muco-enteritis is, I have reason to believe, often mistaken for typhus; in each disease there is the same muscular prostration; but in muco-enteritis there is little or no cerebral affection, which, from the beginning, is usually characteristic of typhus. In muco-enteritis the patient, from mere sluggishness, may be disinclined to speak; but in typhus the symptoms are graver; there is commonly pain in the head, and the sluggishness assumes the character of stupor, from which we must rouse the patient, and even then find it difficult to make him comprehend our questions. As a general rule, the head is not affected in muco-enteritis; in typhus it is commonly so; and this symptom, with attention to the state of the tongue, forms a certain diagnostic of the two diseases.

Muscular Relaxation.

The stage of typhus in which, to my apprehension, the muscular system is most decidedly influenced by the condition of the nervous centres, is that of relaxation; and this state of the muscular system is the result of pressure on the brain, and accompanies the state of relaxation of the sphincters and dilatation of the pupil, attributable to the same cause.

The iris, on account of its greater susceptibility, may be affected earlier than the other muscular organs, but the cause by which it is influenced is the same, and it consequently forms a good index of the state of the muscular system at large. There is no general diagnostic so satisfactory to my mind, in forming an opinion of the power of the system, as the state of the muscles. If, for instance, we were asked what symptoms in the last stage indicated most danger, we should infallibly answer,—a dilated pupil; falling of the upper eyelid; subsultus tendinum; involuntary excretions; and the supine posture: symptoms of muscular relaxation. These are the effects of a morbid state of the nervous centres, wherein there is an annihilation of nervous energy. This leads us to the subject of the cerebral lesions in typhus.

Cerebral Lesions.

Congestion and inflammation of the brain and its meninges, are two of the most important pathological conditions in typhus. This is a point that has been so attentively studied and so well established by the observations of physicians of great reputation and experience, that I shall dwell upon it only to show the peculiarities that distinguish these lesions. According to Louis, whose observations are the most copious and accurate with which I am acquainted, the substance of the brain itself is most commonly the suffering part, the medullary portion being injected in six-sevenths of his cases; the cortical substance exhibiting preternatural vascularity in three-sevenths only. In rather more than half his cases there was unusual redness of the arachnoid and pia mater; the dura mater was seldom affected. It is worthy of observation that the injection of pia mater and brain was greatest in those cases in which death ensued quickly, but in those that lingered long before dissolution effusion was most common. In a few cases the brain was either denser or softer than natural, but this effect was always general to the mass, and therefore, as Louis concludes, not the result of inflammation. Let us compare the foregoing summary of the statistical accounts of the affection of the brain in typhus, by Louis, with the results of 46 cases of cerebritis collected by Lallemand. Of these, 33 exhibited signs of previous inflammation of the cortical structure, and only 8 of inflammation of the medullary portion; in 16 cases the cortical structure covering the convolutions was principally affected; the corpora striata and optic thalami in 13; the tuber annulare in 4 only.

Are the Cerebral Lesions Congestive or Inflammatory?

In cerebritis the cortical structure is affected in proportion to the medullary, as four to one; in typhus the proportions are reversed, and the affection of the medullary portion is to the cortical as two to one. Whence this discrepancy? The question naturally arises, Is the affection of the brain and membranes in typhus generally of an inflammatory nature? I answer in the negative. There are several points of difference between acute cerebritis and the typhoid affection. In cerebritis the injection is generally limited to particular spots; in typhus the injection is usually general. In cerebritis adjacent parts are, according to the stage of disease, either denser or softer than natural, with occasionally serous or purulent infiltration and apoplectic spots; these lesions are but seldom found in typhus; in cerebritis the effused fluid is more or less opake; in typhus it is generally transparent. These particulars are sufficient to enable us to draw a clear distinction between the two affections, and authorise us to consider the cerebral lesions in typhus as the effect of congestion rather than of inflammation.

The effusion discovered after death is no certain argument in favour of previous inflammation, for we all know is that towards the termination of many diseases the serous

and mucous surfaces exhale fluid, and sometimes in great abundance. Even the skin will do this; and, on the approach of death, in typhus the effluvia from the skin will be sometimes increased to such a degree as to be scarcely tolerable to the by-stander. It is more particularly to be observed that effusion into the ventricles or between the membranes takes place in typhus more commonly in those cases that have lingered for a considerable period; and this fact tends to prove that the effusion is passive, and is the effect of want of tone of the secreting vessels.

Practical experience is confirmatory of these pathological facts. Inflammation of the brain or membranes has been denoted with clearness only at the commencement of typhus; and if the patient die at this period the traces of inflammation may be detected. But, by far the greater number of cases exhibit symptoms of congestion alone from the beginning, and these run through a longer course, and when they terminate in death signs of general congestion and effusion are discovered. It is certainly very difficult to diagnosticate, in cases of low fever, between simple congestion and subacute inflammation of an organ, because each frequently terminates in the other, and creates a complication of symptoms; but the signs, as regards the brain, are sufficiently clear. In inflammation of the membranes the pain is more intense and darting, with a sense of fulness and weight at the top of the head; the pupil is contracted, and the eyes are suffused and avoid the light. The least noise is usually distressing to the patient; pyrexia greater. All the senses, but especially those of hearing and sight, are morbidly acute. In congestion the senses are but slightly affected at first, and are usually less impressionable throughout the disease, and the fever is of a lower type. The neurilema of the nerves of sense is probably affected in inflammation of the membranes of the brain, but is intact in mere congestion. The eye is a good criterion of these several conditions of the cerebral organ. In a state of congestion it is dull and hazy, and the pupil is sluggish, and this is the more common appearance in typhus. To this dull state of the eye a clear glistening brightness sometimes succeeds, but it must not be mistaken for the suffused and injected eye of inflammation; it is, on the contrary, a symptom of hectic, and is pathognomonic of suppuration of the glands of the ileum. The hectic stage of typhus is never noticed by authors, but in some cases where the necessary pathological conditions are present, it is as well marked as in phthisis. Such cases will always betray symptoms of suppuration, either of tubercles of the lungs, abdominal glands, or both; but I shall discriminate these points more closely when I treat of the various stages of the disease; and I mention it now merely to show the just value of the eye as a diagnostic symptom. The glassy eye occurring in typhus often deludes the observer into hopes of returning health, and its indications receive an apparent corroboration from the clean tongue and flushed cheek by which it is accompanied; but these are usually fatally symptomatic of extensive disease of the glands and mucous surface.

Mechanical Cause of the Congestion and Effusion.

I believe that actual inflammation of the brain and membranes is not common in typhus at any stage. Of twelve cases now under treatment, I have only one in which I can unequivocally declare the existence of this lesion, and in my previous practice I have not found the proportion to be greater; in all, however, there has been congestion of this organ. But the wonder is not that the brain is congested, or that the pia mater is loaded with blood; it would be, indeed, a wonder if it were not so when every other organ is in the same state. The congestion is an effect and not a cause of the disease, and practically requires, in the majority of cases, no specific treatment. Many of the symptoms and post-mortem appearances in typhus may be attributed to mechanical causes. The brain, like every other fatty and albuminous part of the body, is subject to rapid waste in this disease; and as the cranium is an unyielding structure, the diminution of the volume of the brain must tend to produce a vacuum between the membranes. This tendency alone would cause pain and weight, symptoms always present after excessive loss of blood, and would, as in the application of a cupping-glass, attract a sufficient quantity of blood to fill the increase of space. Either a state of congestion would be thus produced, or fluid would be effused, as another mode of maintaining the integrity of the mechanical forces, and of preventing undue pressure of the cerebral organ. I consider this as an undeniable law, so that in every case of confirmed typhus disturbance of the brain may be expected. Whenever the brain diminishes much in volume, as in old people, or during rapid emaciation from fever, we may look forward to congestion or effusion; and this view of the subject affords a key to the explanation of many diseased conditions of this organ hitherto not well understood.

State of the Lungs.

It is necessary to comment upon the state of the lungs in this disease; for although, during the fever, they do not usually exhibit very prominent local symptoms, yet there can be no doubt that the cure is retarded in proportion to their congestion; and their various affections form some of the most important sequelæ of typhus. During the progress of fever congestion of the lungs is a constant symptom, and is usually accompanied by

bronchitis of a subacute character. The congestion may terminate in pneumonia, or call into activity dormant tubercular deposits, and thus occasion a succession of symptoms that may eventually present greater danger than the original disease. So far as my observations have extended, those cases that were most intractable under recovery were such as betrayed a phthisical affection of the lungs. These cases were generally accompanied also by more acute gastro-enteritic symptoms. With regard to bronchitis I may observe that I have lately had one case of a child, ætat. 4, who had apparently recovered from typhus, but in about ten days after he was considered free from danger the inflammation extended to the larynx, and he suffered from a violent attack of croup. This sequela was arrested by the action of the appropriate remedies, but it terminated in debility, causing the death of the patient. This is the only case of croup I have ever witnessed after typhus, although it is not uncommon during or after measles. The reason is to be found in the less acute character of the affection of the lungs in typhus.

The post-mortem appearances are congestion and inflammation, and their effects, such as induration and infiltration, either serous or purulent; in addition to these the usual characters of phthisis are occasionally observable.

Every organ is affected during typhus, because typhus is common to every organ. But if I were asked at what period of the disease the various lesions of each organ were most obnoxious to the patient, I should reply that the lesions of the brain were most to be dreaded during the fever; the affections of the lungs during the convalescence; but that the lesions of the digestive apparatus required urgent attention through every stage of the disorder, until the re-establishment of health. These organs are invariably diseased. The spleen, for example, according to my own experience, and the unanimous testimony of medical authors, is, at all times, found in a state of softening; but it does not appear that this state is generally the result of inflammatory action. Dr. Tweedie, however, has adduced one case that occurred at the London Fever Hospital, in which an abscess was found in the spleen, but in connection with inflammation of the serous covering of the diaphragm. The exception proves the rule. The spleen being a very distensible organ, would easily yield to the pressure of an accumulating mass. The liver, too, is very generally in a state of congestion; inflammation occasionally supervenes, and softening, not commonly the result of inflammation, is frequent.

State of Lymphatic System.

Those components of the digestive system whose lesions seem to be more particularly distinctive of typhus, are the various glandular organs of the intestines and the lymphatics at large. Every other organ seems to suffer primarily under congestion alone, followed by various sequelæ dependent on the severity of the fever and the peculiar constitution of the patient; but the glandular system is affected from the first by the action of the virus, and undergoes certain changes that maintain and augment the degree of constitutional disturbance. It was this almost invariable affection of the glands of the gastro-enteritic surface that led Broussais to form his famous theory of the cause of the disease,—a cause which later observers, especially in this country, have found to be sometimes wanting, and therefore not essential to the malady. Broussais erred, then, in limiting the pathology of typhus to these glands alone. These glands form only a part of a system whose office it is to prepare the food for nutrition; and although these glands in particular may not be always diseased, yet the system of which they form a part will be constantly found affected.

Identity of Function of the Abdominal and General Lymphatic Systems.

In announcing that the mesenteric glands, the lymphatic glands, and the glands of the intestinal mucous surface perform the same functions, I am aware that I shall be considered as hazarding an opinion not sufficiently proved; but I feel persuaded that the data furnished both by physiology and pathology sanction and confirm this view of their actions. During mesenteric disease, for example, induration and suppuration of the intestinal glands are very common; in fact, it is a great question whether, in the last stages of mesenteric disease, these glands are ever free from disease. According to the observations of the most accurate observers, disease of the glands of the mucous surface of the intestine is constantly found in post-mortem examinations of patients who have died of what is commonly called mesenteric disease. I have lately had a most beautiful instance of this lesion, in which primary induration of these glands was very perceptible beneath the transparent mucous membrane, showing that this membrane was only secondarily affected. It is now fully proved that mesenteric disease, simply as such, never causes death, but that this is immediately the result of affection of the mucous surface of the intestine and its glands. In phthisis, also, disease of the mesenteric glands, and of the glands of the ileum, are generally found co-existent. These are collateral proofs.

State of the Lymphatic System in Typhus.

Typhus exhibits this relation almost invariably; the two lesions generally accompany each other; and I much doubt if both are ever wanting. Although inflammation,

softening, or suppuration of the glands of the ileum, may not be always present, yet disease of the mesenteric glands is seldom absent; and this position is the result of a comparison of the most extensive observations of English and foreign pathologists. The general absorbent system is invariably either functionally disordered or structurally diseased in typhus, as is proved by the rapid emaciation of the body, and by the affections of the external glands occurring as frequent sequelæ.

Although disease of the ileo-intestinal glands exhibits more severe symptoms than that of the mesenteric glands, on account of their contiguity to the mucous surface, yet it is not more pathognomonic of typhus, nor is either of these of more consequence in this point of view than the affection of the general absorbent system. Broussais erred in two modes, in one as to fact, in the other as to inference. In the fact, by a partial and exclusive statement that lesion of the ileo-intestinal glands was the only pathological condition essential to the disease, whereas affection of the mesenteric glands is even more constant; and in the inference, that the fever was only irritative, or secondary to the local affection.

When the glands of the mucous surface are in a state of suppuration the minute plexus of vessels and absorbents on the mesentery, within about a quarter of an inch of its duplicature to enclose the intestine, is more injected than usual, and the absorbent vessels leading from this plexus are generally found of a dull-white colour, being evidently filled with purulent matter. This I have frequently observed, and have been thus convinced that the absorbents passing from these diseased glands are affected by the same inflammatory action, and secrete pus. The lacteals pass on to the mesenteric glands, and thus there is a continuous chain of inflammatory action. This inflammation of the lacteals is of precisely the same nature as that occurring from the application of a poison to the surface of the skin. It matters not how the poison be taken up, the effect upon the system is universally the same. Thus, tartar emetic injected into the veins produces nausea and vomiting; mercury rubbed into the skin produces salivation; in fact, every medicine and every poison received into the system has a specific action; and thus it is that the poison of typhus, however introduced, causes inflammation of the absorbent lymphatic system of the abdomen.

The consequence of this condition of the lacteals is anorexia, and loss of power of the digestive functions, to remedy which the systemic absorbents commence taking up the fatty matters in the cells of the adipose tissue; but these principles do not undergo the necessary changes, and subsequently inflammation of the systemic absorbents is frequently established. The increased activity of the systemic absorbents is one of the most prominent characteristics of the disease, and exists, doubtless, in consequence of a demand made upon the unorganised matter of the system to supply the waste rapidly carried on by the actions in the extreme texture. I believe, also, that the whole absorbent system suffers an augmented activity from the influence of the poison, and that the action of the systemic absorbents is not entirely secondary to the affection of the glands of the abdomen; for in plague, a disease which bears a striking analogy to typhus, the inflammation of the absorbents is the pathognomonic symptom. The black death, also, a disease that depopulated Europe during the middle ages, and which strongly resembled, and was perhaps identical with the plague, was characterised by the supervention of buboes or abscesses of the lymphatic glands of the general system.

From an attentive consideration of the pathological conditions of the absorbent system in various diseases, I have been led to form the opinion that the lacteals and the lymphatics of the system perform one and the same office, namely, the preparation of food for the purpose of nutrition. It was the opinion of Hunter, and it is one still generally credited, that the lymphatics absorb the waste matter of the system. Magendie, on the other hand, denies to the absorbents, so called, the power of absorption, while Müller declares that they receive the liquor sanguinis direct from the capillaries, a view prohibitory of absorption, yet he does not deny it altogether. It was on account of the qualities of the fluid found in the lymphatics that Müller proposed this theory; but he neglected, in his estimate, to consider the proportions which these principles bear to each other. As it is utterly impossible to entertain clear notions of the pathological conditions of any disease, without having a previous knowledge of the same parts in a state of health, I shall briefly enumerate the peculiarities of the fluid in the absorbent vessels.

Identity of Function of the whole Lymphatic System proved Physiologically.

In opposition to the authorities above named, I have stated that the functions of the lymphatics are precisely the same as those of the lacteals; that they do not take up waste matter, or the effete particles of the body, and that they are not merely the vehicles of the colourless liquor sanguinis. It is further apparent that the glands through which the absorbents pass assist in the preparation of food for the system, in the same manner as the mesenteric glands of the abdominal cavity; in fact, that the lacteals and lymphatics, with their glands, form one complete digestive system. This must be the inevitable conclusion derived from an ex-

amination of the relative proportions of the principles of the fluid contained in these vessels.

Chemical Constitution of Lymph.

Oily matters, albumen, and salines are the principles in excess in healthy lymph. If we draw a comparison between the relative quantities of each of these principles contained in the lymph and blood respectively, we shall be sensible of the great difference. According to Lecanu, 1000 parts of blood contain from 1.20 to 2.10 of crystallisable fatty matter, and from 1. to 1.30 of oily matter; if these be added together, and the mean taken, it amounts to about 3 parts in 1000. Fibrine, of all the constituents of blood, contains the greatest relative proportions of oily matter, and this amount, according to Chevreul, does not average more than 4 or 5 per cent. Now, Marchand and Colberg, who have analysed the lymph very carefully, state that 1000 parts of lymph contain 2.65 of oily and fatty matters, being only one-eighth less than the proportion contained in the same quantity of blood. This is the absolute ratio in equal amounts of blood and lymph respectively, comprising the aqueous and solid materials in their natural proportion. But if we subtract the aqueous particles, which may be considered merely as the "ventriculum," and examine into the relative proportion of the oily matters to the other solid ingredients, we shall find a very material difference. This is, in truth, the correct mode of examination.

Amount of Solid Matters in Blood and Lymph respectively.

According to Prèvost and Dumas 1000 parts of blood contain 129.20 of coagulum, which, compared with the oily matters, represented as 3, is as 43 to 1. But 1000 parts of lymph contain of fibrine 5.20 parts, which amount is to the oily matters in the same quantity of lymph, represented as 2.65, only as 2 to 1. In fact, there are 43 times more fibrine than oily matters in the blood, and only twice as much in lymph; consequently a very great relative excess of oily matters in the latter. Again, according to Prèvost and Dumas, 1000 parts of blood contain 86.90 of albumen; according to Marchand and Colberg, 1000 parts of lymph contain 4.34 of albumen: thus, the proportion of fibrine to albumen in the blood is as 1½ to 1, or one-half more; in the lymph as about 5 to 4, or scarcely one-fourth more. This proves, therefore, that lymph contains, for the same amount of solid ingredients, twice as much albumen as blood, and twenty-one times as much oily matters.

Is there no design in this disposition of the organic principles contained in the lymphatic vessels? Does it not rather tend to show that the lymphatics are not mere organs of transit for the colourless liquor sanguinis; and that they do not take up simply the waste matter of the system; that, contrarily, they are true digestive organs; that in them the oily matters undergo a gradual change, and are converted into albumen, and this also into fibrine? The oily matters may be considered as the basis of the principles of nutrition; for, with the exception of the fibrine and albumen taken into the system ready formed, it is well known that the greater proportion of the other ingredients is compounded of starch, which, before assimilation, is converted into oily matter. Now, in the same way that the lacteals digest the oily matters formed from the starch of our food, the lymphatics digest the oily matters absorbed from the system. The adipose tissue is, in truth, a magazine of food, of which the lymphatics are the digestive apparatus. The analogy between the lacteals and lymphatics, as to their contents, is equally striking, oily matters and albumen being in great excess in the latter, and in the former fibrine does not appear until they have received the lymph from the spleen, and passed through the mesenteric glands.* The conversion of oily matters into azotised principles cannot be carried on unless there be a proper supply of such principles from the blood; for which reason the absorbent and mesenteric glands are abundantly supplied with arterial blood. All these glands, having no ducts, doubtless fulfil the same office.

Assimilation in Hybernating Animals.

In the case of hybernating animals we find a strong confirmation of the truth of the principle sought to be established. When these animals retire for their winter sleep their adipose tissue is loaded with fat, which, on their awakening from their dormant state, is found to have been entirely consumed. The question arises, to what use has their fat been applied? Liebig, in his highly ingenious work, states his belief that the fatty matter has been wholly employed in the maintenance of the function of respiration; but, with all due deference to so high an authority, I may ask by what means does the animal subsist during this period? how is the waste in its system replaced, so that life may be supported? It is well known that the secretion of bile and urine continues under these circumstances, and is consequent upon the disintegrating actions going on in the extreme texture. These actions certainly could not be carried on, however slowly, for any length of time without producing disease or death, unless new increments of azotised principles were constantly conveyed to the system. Respiration alone cannot maintain life, rather

* See this relation explained in my paper on the Physiology of the Spleen, contained in THE LANCET of Jan. 4th.

would it tend to destroy it. It is, however, by the gradual, though slow, conversion of fatty matter into albumen and fibrine, that the chemical and vital actions are maintained in their integrity. Liebig, also, quotes Martell, to state that a fat pig was buried for the period of one hundred and sixty days under a mass of earth, and at the end of this time was found to have lost one hundred and twenty pounds in weight. Is it possible that a pig could live for so long a period as five months and ten days without food? I do not mean to doubt the truth of the statement; on the contrary, I admit it, and believe it to be one of the strongest evidences of the gradual conversion of oily matters into azotised principles. In affirming that an animal lives upon himself under such circumstances, I say no more than has been vaguely said for an indefinite time past; but I wish to give the expression force and meaning, by exhibiting a rationale of this mode of assimilation, and by determining the organs that are instrumental in conducting the process.

Change of Oily Matter into Albumen in the Egg.

The process of the conversion of oily matters into albumen is beautifully exemplified in the egg. In the early stages the quantity of the white is small, and the yolk great; now, the yolk contains a large proportion of oily matters in union with albumen; but gradually the white increases in amount, and, consequently, the albumen bears a proportional increase to the amount of oily matters. The conditions are changed; oily matters have been converted into albumen, and how? As the albumen forms, a space is left, by the disappearance of watery particles, at the large end of the egg; in this space atmospheric air is found, but there is a disproportion in its elements; for, according to the experiments of Dulk and Bischoff, there is an average increase of 4 per cent. of oxygen. We can account for this increase of oxygen only by presuming an absorption of nitrogen; and there can be but little doubt that this nitrogen is employed in the conversion of oily matters into albumen.

The same Change effected in the Human System.

With respect to the conversion of oily matters into azotised principles in the human system, I may adduce the formula contained in my former paper, showing that by the addition of uric acid to oily matter, we obtained the equivalents of the red particles. If also to two equivalents of oily matter we add one and a half equivalent of uric acid, we obtain proteine, fibrine, or albumen, thus:—

	C	N	H	O
3 eq. of oily matter ...	33		30	3
1½ eq. of uric acid	15	6	6	9
	48	6	36	12

1 eq. of proteine minus two atoms of oxygen.

Whence it appears that uric acid, or its elements, is a most important agent in the assimilation of the oily matters that form the basis of certain parts of our food. These changes in the digestive process are, I believe, carried on chiefly, though not entirely, in the lymphatic system, combining thus the lacteals and external lymphatics with their several glands. This view of the function of this system satisfactorily accounts for the large quantity of arterial blood which it receives. I think that there is also sufficient evidence to authorise us to ascribe the same function to all those glandular structures unprovided with efferential ducts. But this point may be still considered *sub judice*.

We must be careful not to confound this elimination of uric acid, and its combination with oily matters in the absorbent system, with the general formation of uric acid in the extreme texture; not but that I believe that the same changes may occur to a limited extent in the circulating mass, but the conversion of oily matter into albumen is so gradual that this use of oily matter in the circulating current bears no proportion to its proper office of uniting with oxygen to form water and carbonic acid in the production of heat. Dr. Prout has shown that after a full meal of oily matter the urine contains an unusual proportion of uric acid, which Liebig accounts for by supposing that the oily matter combines with a greater portion of oxygen, and withdraws an equal portion from the quantity necessary for the maintenance of the healthy action of the forces in the extreme tissue, and the consequence is the formation of uric acid. This, as a general rule, is no doubt true. The uric acid formed in the extreme texture is properly a substance for excretion, for the oily matters with which it is brought in contact in the circulation have a greater affinity for oxygen than for itself, and therefore those changes, for the purposes of nutrition, which I have ascribed to the function of the lymphatics, do not take place in any appreciable degree.

It is wrong, however, to consider uric acid, or its elements, as a mere excretion. It is a most important azotising principle, and particularly essential in the economy of all animals living on a herbivorous or mixed diet.

I have thus briefly developed my views of the function of the lymphatic system, in order that the reader may be enabled to understand more clearly the doctrines announced respecting the pathology of typhus. It is absurd to attempt to understand the

pathology of any disease without comprehending the premises furnished by the physiology of the organs which it implicates. The function of the absorbents is dependent on the condition of the blood. During the period of increased action in the early stage of typhus there is found an increase in the amount of the coagulum of the blood, which is firm and tenacious; at a later period there is a diminution in the quantity of salines and aqueous particles, and the colouring matter is of a black hue, and often precipitated at the bottom of the vessel; lastly, coagulation scarcely takes place at all, but the blood is a black, or greenish-black, soft, tarry-looking mass. These appearances have a direct reference to the action of the lymphatics.

It is highly probable that in the first stage of fever a considerable portion of the oily matters supplied by the vigorous action of the lymphatics combines with uric acid to form fibrine. This is the stage of increased action. As the disease advances, however, the function of the absorbents becomes suspended, and the oily matters pass off with the urea in greater amount. Lastly, inflammation and suppuration arise, and constitute the pathology of typhus.

Diagnosis of the various Kinds of Petechiæ.

As I have adverted to the various changes that take place in the blood in this disease, I shall here remark upon the hæmorrhagic tendency which usually exists in severe cases. The organs that generally exhibit this symptom are the skin, lungs, and bowels. Petechiæ are of various kinds, but for practical purposes it may be necessary to distinguish three only. In the first kind the petechiæ are of a bright red colour, of the size of a pin's head, and are often mistaken for fleabites, from which they may be distinguished by not having a central dot and areola in the circumference. These commonly occur on the neck and breast, at about the fourth day of the disease. If the fever continue mild they retain their colour throughout the malady, but if towards the termination they become of a livid hue, death is usually the result. The more sudden the change the greater the apprehension. This change will sometimes take place within twenty-four hours, and in such instances there is but little hope for the patient. Petechiæ of a bright red colour, then, do not authorise an unfavourable prognosis, but livid petechiæ threaten great danger, and generally in proportion to the suddenness of the change. In the third kind the petechiæ are called vibices, and are of a large size. Their colour is livid or greenish-black, and whenever they appear, whether early or late, death may be anticipated. In diseases of excessive violence, as the plague, these vibices appear early, as well also in some very severe forms of typhus; but in this disease, under its ordinary form, they are seldom observed until near the termination, when decomposition of the blood has taken place. During an endemic typhus lately observed, there were no cases of petechiæ, with the exception of those that occurred in one family, seven of whom fell ill of the disease, and all displayed petechiæ at an early period. In this instance the presence of the petechiæ depended upon the hæmorrhagic constitution of this family.

Hæmorrhages.

Hæmorrhage from the bowels is not uncommon in fatal cases, and the quantity of dark decomposed blood sometimes poured out from the mucous membrane is surprising. I remember one case that happened a few years since, of a young woman, sixteen years of age, from whose bowels nearly three urinals full of blood were thrown out previously to death. These cases are beyond the aid of medicine.

Hæmoptysis occasionally occurs as a symptom of typhus, and evidently depends upon the same causes as hæmorrhage from any other organ, namely, decomposition of the blood, and loss of tone of the capillary vessels. In certain forms of typhus there is a greater tendency to hæmoptysis than in others, just as, during certain epidemics, typhus will be so commonly attended with petechia as to receive a specific designation from this symptom; whereas during others it will never show this character. These are peculiarities that our knowledge of the laws of disease is not yet sufficiently copious or accurate to explain. The season of the year and temperature, no doubt, influence them in a great degree; but there are no statistical facts as yet ascertained that can give authority to conjecture.

ELECTRO-PUNCTURE.

M. Schuster has lately addressed a paper to the French Academy of Sciences, to prove that electricity is little serviceable in medicine unless it be applied through the means of acupuncture-needles. Administered in this way, he asserts it may be employed with success against dropsies of all kinds, steatomatous and other tumours, engorgements and indurations (and perhaps cancer), goitre, varicose dilatations, chronic rheumatism, paralysis, amaurosis, and perhaps aneurism. Electro-puncture acts, he says, by directly stimulating the sensibility, contractility, and absorbent function; in the formation of internal eschars, and the detraction, piece by piece, of diseased growths; in decomposing the fluid contents of tumours; in procuring external openings for matter, provoking adhesive inflammations, &c.; though some of these effects seem capable of production by the needles alone without the electricity.

UNIVERSITY COLLEGE HOSPITAL.

PECULIAR CASE OF PULMONARY DISEASE.

Collection of matter in abdomen, between spleen and diaphragm; adhesion of latter to left lung; inflammation of lung; several circumscribed abscesses; perforation of both pleuræ; hydro-pneumothorax of left side; tubercles in upper lobe; recent pneumonia and pleurisy of right lung; ulceration with perforation of stomach. Physical signs: metallic tinkling; amphoric respiration; change of resonance on percussion; crepitation and friction sound.

Case.—Mary Ann Filler, aged 20, was admitted Sunday, Oct. 23, 1842, under Dr. Taylor. She was of slender conformation and pale complexion; a servant of all-work, single. Her work has been very arduous. She has always had enough to eat, and food of good quality. Resides near Gray's-inn-road, in a dry and open situation. Her mother died fifteen years ago; she does not know of what disease, but it was of short duration. She was never subject to cough or any other complaint. Her father is still living, subject to the gravel, but to no other complaint; is not subject to cough. She has nine brothers and sisters, all enjoying good health. She considers herself habitually healthy, but not strong. She has not been subject to rheumatism or any other complaint. She never had a cough until last October or November, when, after going out in a fog at night-time, she caught cold, with a sore throat. She recollects distinctly catching cold; a cough and pain at the sternum followed. She did not expectorate anything. She has never spit blood at any time of her life; this attack continued five weeks. In March last she was similarly attacked for a fortnight, from getting her feet wet. On Friday fortnight, October 7th, she was suddenly attacked with shortness of breath and slight pain in the right side of the chest. She was previously quite as well as usual up to the moment of the attack. She had been working very hard during the previous week, and did not get to bed till from twelve to two o'clock in the morning. She was not exposed to cold or wet during the time. She describes the pain in the side as stabbing and cutting; it was increased upon motion, or upon taking a deep breath; she had no cough at first. On the Saturday she was bled by a medical man; on the Tuesday a blister was applied to the side. The blister relieved the pain, and she found herself considerably better. On Friday, October 14th, was seen by Dr. Taylor, when, on a cursory examination, the following signs were observed:—*Left side* duller behind on percussion than right; bronchial respiration below the scapula; vesicular respiration feeble, or wanting; no change in respiration or percussion under clavicles. There was pain in the left side; very slight cough; a little mucous expectoration; considerable dyspnœa; debility; pallor; emaciation. She was considered to have pleurisy, and had much the aspect of a patient with phthisis, but the existence of this disease was not ascertained. The gums had been affected with mercury for two or three days, when, on Wednesday or Thursday, she "took cold," got sore throat, increased cough, and with it dyspnœa. She was attacked with the same kind of pain in the right side of the chest; no sudden pain in left side, but a sudden increase in dyspnœa. She gradually became worse until her admission.

At present dyspnœa is extreme; is propped up with pillows; slight pain in the right side; not much cough. A deep breath increases the pain; it is not so bad as at first, but her breath is shorter; no pain in left side; expectorates a thick serum, or thin pus, of a dirty-grey colour, and intensely fœtid. She commenced to expectorate this on Wednesday or Thursday last. Pulse 140, small, soft; respirations 34; lips livid; countenance very anxious; emaciation considerable; slight hoarseness; bowels regular; surface of the body warm and moist; no headach; no pain in abdomen or sickness; urine rather high-coloured and turbid, neutral, not albuminous. Physical signs:—*Left lung* seems to expand rather less freely than right; in inferior part intercostal spaces seem less depressed than on right side; percussion anteriorly quite dull in lower half or two-thirds, duller than natural in upper one-third, but much less dull than below; rather less dull, perhaps, under left than right clavicle; behind duller than natural in supra-spinous fossa; in infra-spinous fossa clearer than on the right side; below the scapula duller on the left than right. *Respiration*: anteriorly, in upper third, some muco-crepitant rhonchus; in lower half, or two-thirds, large muco-crepitant rhonchus, or gurgling; and immediately under left mamma the gurgling is larger, and has a metallic tone, resembling metallic tinkling (the metallic tinkling occurs in the interval of the heart's sounds); respiration has occasionally a cavernous or amphoric character. Posteriorly, vesicular murmur feeble in upper and lower parts of lung; a little muco-crepitant rhonchus in the upper part; at lower angle of scapula, and near it, there is a metallic-toned gurgling, and amphoric rather than bronchial respiration.—*Right lung:* Percussion, anteriorly, quite dull in lower two-thirds; about the mamma, between it and sternum, a distinct bottle resonance; in upper third duller than natural; behind, dull in supra-spinous fossa; duller in infra-spinous fossa than left; below the scapula clearer than left.—*Respiration*, anteriorly: some muco-crepitant rhon-

chus under the clavicle; in lower half large crepitation of all sizes in various parts, in some approaching gurgling, but everywhere with a *crepitating tone*. Posteriorly, some muco-crepitant rhonchus in upper third, in lower two-thirds, almost universally, a double friction sound, synchronous with inspiration and expiration, the latter rather louder; some distant muco-crepitant rhonchus in same situation; the hand applied to the chest perceives a distant purring vibration over the seat of rubbing sound. Heart's sounds not morbid; impulse perhaps a little increased. Was ordered yesterday, by Dr. Quain—℞ *Tartarised antimony*, one-eighth of a grain; *chloride of mercury*, a grain. Mix with conserve for a pill, to be taken every six hours.—℞ *Carbonate of ammonia*, four grains; *camphor mixture*, an ounce. Mix for a draught, to be taken if required.

Diagnosis dictated at first visit.—Pleurisy of left side; perforation of pleura; hydropneumothorax; secondary pneumonia, with condensation of lower and anterior two-thirds of right lung; pleurisy of right side, with effusion of fibrin, but no effusion of serum or very trifling; probably tubercles in both lungs.

25. Had great shortness of breath during the night; this morning she feels somewhat better. On measuring just below the mamma the whole circumference of the chest was twenty-seven inches, the right side being fourteen; pulse 130; lips less livid to-day. Medicine purged her much during the night, five or six times. She says she can lie on either side, but with difficulty; lies better on the left than the right side. On percussion the sound is perhaps duller under the right than left clavicle. Expectoration thicker and less fœtid; a slight murmur at the base of the heart with the first sound.—℞ *Compound powder of kino*, a scruple, to be taken twice a-day. Add to each dose of the ammonia mixture two minims of oil of spearmint.

26. Feels better; breathes more freely; can talk louder; metallic tinkling more distinct to-day; expectoration thicker, and not so fœtid; pulse 168; cannot sit up, being too much exhausted; syncope soon comes on when she is placed upright; no purging since she took the powders. Ordered beef-tea, a pint.

27. Feels much better; countenance not so anxious; lips less livid; face flushed; cough troublesome, prevented her from sleeping during the night; no sickness; was purged four or five times during the night; pulse 116; *aphthous tongue*; respirations 32; the right and left sides of the chest expand equally; breath fœtid. Beef-tea, a pint and a half.

28. Was purged much during the night; did not sleep; feels better; large crepitation, reaches up nearly to the clavicle on right side; fine crepitation heard posteriorly on right side, and still some friction-sound; skin very hot posteriorly; tongue cleaner; throat feels sore; vomited the beef-tea during the night; has a great disposition to faint from weakness; frequent flushing of the face.—℞ *Chalk mixture*, an ounce and a half, to be taken when necessary.

29. Did not sleep during the night; was purged; coughed much; has great shortness of breath this morning, otherwise does not feel worse; lips not so livid; pulse 134; respiration 36.

30. Died at a little after ten o'clock, a.m.

Post-mortem Examination Twenty-eight Hours after Death.

Oct. 31st, 1842.—Exterior: emaciation moderate; on percussing the chest there was elicited a healthy pulmonic resonance for some distance below each clavicle. Over lower half of left side, in front, there was an amphoric resonance. The depending part of left lateral surface dull. Lower half of right front dull, except at inner side of, and rather below, the mamma, where there was an amphoric resonance, but less diffused than on left side. An aperture was made in one of the intercostal spaces of left side, and a lighted candle placed near it, but no current of air was observed to escape; a fœtid odour, however, was perceived, resembling that of the expectoration.—*Chest.* Left pleura partially attached to pleura costalis by moderately firm adhesions, of a red colour; adhesions also between pleura and pericardium. The summit was free from adhesion, also the whole of the upper lobe, except a small portion laterally; the lower lobe was partially adherent. There was a small amount of fluid in the pleura, the quantity and characters of which were not examined with care. Some rather recent but roughish lymph on the back part of the lower lobe, where also the pleura was redder than usual. The pleura at the base of the lung was universally connected to the upper surface of the diaphragm by very firm old adhesions, so close that the diaphragm appeared like a thickened pleura. The abdominal surface of the diaphragm was firmly connected to a portion of the circumference of the spleen (the large convex surface of which was turned towards it), so as to enclose a cavity between the two, which contained a quantity of pus (perhaps three ounces); at first the cavity was supposed to be situated between the base of the lung and upper surface of the diaphragm. In the upper and outer corner of this cavity the pleura and diaphragm were both destroyed over a circumscribed space, exposing the pulmonary tissue, which also was somewhat excavated, and had a dark-coloured uneven surface, covered by an ash-coloured false membrane. This space was crossed partially by bands, like obliterated vessels, or bronchial tubes. In this part there was a

distinct communication between the cavity just described and the general cavity of the pleura, by a circular aperture, about a line in width; the surfaces circumscribing the cavity between the spleen and diaphragm were corrugated, and covered with a *thick* and *firm* layer of false membrane. The lung was inflated as well as possible by means of a tube, passed into the trachea and bronchi, but very little beyond the upper lobe could be expanded, and no air could be made to pass from the lung into the pleura by the aperture already described. The lower lobe of left lung was somewhat diminished in volume—dense, dark, red-coloured, non-crepitant, and more brittle than natural. At the back part of it, and about the middle of its perpendicular depth, was a cavity capable of holding a small walnut; it was placed immediately beneath the pleura, which was thickened and opake, and formed its external wall. There was here no adhesion to the pleura costalis, and the pleura did not rise above the level of the adjacent portions; the other boundaries of the cavity consisted of pulmonary tissues, in which it seemed to have been excavated. The tissue presented an uneven surface, and was covered by a distinct ash-coloured false membrane, of moderate thickness; immediately beyond this membrane the tissue of the lung had the same appearance as the rest of the lobe. Several small openings were observed towards the lung, but no air could be made to escape through them. This cavity was moderately filled with a thick serum, or thin pus, having exactly the odour and other physical characters of the matter expectorated at the time the patient was admitted; several small bands, like vessels or tubes, crossed from one part to another of the sides of the cavity. The two lobes were connected by moderately firm adhesions; the lowest part only of upper lobe was condensed, and in the corner of it was a small cavity lined with false membrane. In the very centre of the apex of lung, about three-quarters of an inch from the summit, was a mass of crude tubercle, the size of a hazel-nut, surrounded by a cyst; a small number of miliary tubercles scattered through substance of this lobe; *no* tubercle found in *lower* lobe.

Right Side.—Partial but soft adhesions between pleura pulmonalis and costalis over lower lobe; firm adhesions of pleura between lobes; pleura covering the whole posterior surface of lower lobe, and a part of upper lobe was roughened by the deposition of a scanty but uneven layer of lymph upon its surface, giving a rough sensation to the finger passed over it; pleura generally redder than usual; a small quantity of turbid serum in its cavity. By firm adhesions of a portion of pleura covering upper surface of diaphragm, adjacent part of costal cartilages, and lower and anterior corner of upper lobe of lung, a small cavity was formed, capable of holding several drachms, and containing a little thick pus. There was a loss of substance in this corner of lung, forming a cavity of the size of a large horse-bean, in the bottom of which was a communication with the lung, through which air could be pressed into the cavity just mentioned. The cavity was bounded on one side by thickened pleura, and towards the substance of the lung was lined with false membrane. The pleura covering the upper lobe was tolerably healthy, and free from adhesions to costal pleura; all but the inferior part of upper lobe crepitant, though perhaps less than natural, and contained a good deal of frothy serum. There was neither crude tubercle nor miliary granulations visible; the lowest part of this lobe, two or three inches in length, was very dense, perfectly void of air, of a general drab colour, dotted over with yellow spots, from which, on compression, pus exuded, leaving a number of very small cavities, lined by a smooth membrane; these were the size of large pins'-heads. The cut surface here was finely *granular*, the substance denser and paler than in red hepatisation. The whole of lower lobe was redder than usual, denser, and less crepitant, but still contained some air. The cut surface allowed some blood to exude, and was of a blood-red colour, deepest near the posterior surface; it was more easily lacerated than in health, but less easily than in ordinary red hepatisation; a portion cut off did not sink in water; no tubercle observed on most careful examination of every part of right lung. Weight of right lung, twenty-seven ounces and a half, left ditto, twenty ounces three drachms; bronchial tubes of both lungs not examined.

Heart.—One and a half or two drachms of clear yellowish serum in pericardium. Heart of ordinary size; left ventricle, walls, and cavity natural. Endocardium opake, and thickened to some extent. Mitral valves and orifice healthy; the latter admits two fingers; one of semilunar valves a little thickened, having a slightly projecting false membrane on ventricular surface. Aorta healthy as far as arch; no spots on it. Left auricle healthy. Foramen ovale closed. Right ventricle and auricle, walls, cavity, endocardium, and valves healthy. Tricuspid orifice admits three fingers.

Blood.—Right ventricle filled moderately with colourless coagulum. Right auricle contained a larger one, of which the back part alone contained colouring matter; left ventricle a small and colourless coagulum. Left auricle a moderate coagulum of black blood. Aorta and pulmonary artery both contained fibrous coagula. Weight of heart, eight ounces six drachms.

Abdomen.—Spleen considerably larger than usual. Besides the collection of matter between it and diaphragm, there was also a smaller one between spleen and lateral part

of abdominal walls, and a third between spleen and large end of stomach; proper coat much thickened; substance rather dark and soft generally, but near to external surface almost black and softer than elsewhere. Weight of spleen, ten ounces three drachms.

Liver.—A small collection of pus between left lobe of liver and anterior wall of abdomen circumscribed by adhesions; left lobe small, substance of liver natural in consistence and colour.

Stomach.—Anterior walls connected in two places to walls of abdomen in front of it by old adhesions; one of these was midway between cardiac and pyloric extremities, and in this spot was a circular perforation, about a line in diameter, but whether cut or torn, or previously existing, was doubtful. There was no recent lymph externally, nor any effusion of contents of stomach. On the corresponding part within there was a circular spot, the size of a silver fourpence, in which the mucous membrane was either destroyed or very thin; the edges of it were rather abrupt but smooth, and there were faint radiating lines in mucous membrane surrounding this spot. There was no redness whatever around. The bottom of this spot was formed of peritoneal coat, a little thickened, and in the centre of this was the circular aperture above alluded to. The second adhesion was nearer to the pylorus, and here, in removing the stomach, a very large opening, one or one and a half inches in diameter, was made, in part, by removing it. The coats of the stomach, however, were here diseased, the serous and mucous lining separated from each other to some extent, thickened, and somewhat red. The mucous membrane in great end was thin and soft, the rest healthy.

Œsophagus.—The lower part, internally, had lying loosely upon it a number of long shreds of a substance, resembling somewhat the pulp which covers the tongue when severely aphthous; under this were shreds of epithelium, loose, but more closely lying on mucous membrane, which, after their removal, was seen to be entire and healthy.

Small Intestine appeared healthy; an ulcer of moderate extent at junction of ileum and cœcum; none elsewhere. Peyer's and other glands healthy. Large intestines healthy; calibre small; mesenteric glands not examined; mesentery tore more readily than usual; no fluid in peritoneum, nor increased vascularity.

Kidneys of rather large size; surface of right one mottled, otherwise healthy; weight of left, seven ounces six drachms; right, six ounces five drachms.

Bladder, uterus, and ovaries, not examined.

Brain.—Several drachms of clear serum in cavity of arachnoid (about six); veins on surface rather empty, and also vessels of pia mater; substance of brain paler than usual, and perhaps softer; no fluid in ventricles. Weight of encephalon, two pounds ten ounces.

*** On this singular and interesting case some valuable clinical remarks were made by Dr. Taylor, one of the physicians of the hospital. These shall be given in the next number of THE LANCET.

EMPLOYMENT OF MURIATE OF AMMONIA IN SCIRRHUS OF THE STOMACH.

THE above salt having been administered by German practitioners with decided benefit in cases of induration and degenerescence of the bladder, prostate gland, &c., Trusen was induced to employ it in the following instances:—

CASE 1.—A man, thirty years of age, accustomed to a sedentary life, and the too great use of alcoholic liquors and strongly spiced aliments, had evidently contracted a scirrhous disease of the pyloric extremity of the stomach. For some months, vomiting, with violent heartburn, &c., had always come on from three to four hours after taking food. Ordered, ammon. hydrochl. fifteen grains, every two hours, combined with extract of liquorice. In a short time digestion was performed better, and the appetite restored. Care and abstemiousness in diet were enjoined, and at the end of a six months' persistence in the use of the medicine, the vomitings ceased entirely. A clogged state of the stomach, from superabundance of mucus and habitual constipation, were removed by a six weeks' visit to the muriated-chalybeate springs of Cudova, and the man subsequently regained perfect health.

CASE 2.—A man of age, and sedentary habits, similar to the foregoing, had been for some time affected with arthritis, &c., on the removal of which he began to be subject to an invariable rejection of the whole of his food a few hours after it had been taken, accompanied by atrophy and emaciation. He entered, however, upon the use of muriate of ammonia in drachm doses (!) which he continued for seven months, when his vomitings had wholly and permanently disappeared.

The system of the patient soon adapted itself without inconvenience to the above large doses of the remedy, which were taken in infusion of ginger.—*Hufeland's Journal* and *Gaz. des Hopitaux.*

GENERAL ABSTRACT
OF THE
VIEWS OF CORRESPONDENTS OF "THE LANCET"
RESPECTING THE
FUNCTION OF THE SPLEEN.

Part I.

The controversy which has been made public in the pages of The Lancet respecting the function of the spleen, has far outgrown the limits we originally anticipated, and now requires the free employment of the editorial axe to chip it down to moderate proportions. Anxious as we are to allow the widest latitude at our disposal to the *writers* in The Lancet, the interests and tastes of its *readers* must also be consulted; for were we to publish entire all the communications on the spleen and its function at present in our hands, many of the latter-named class of persons might think, and justly, that to a subject on which so much that is advanced still remains hypothetical, too much space had been devoted in our Journal. Desirous, therefore, to steer a middle course, we propose in this place to give, in as short a space as practicable, a condensed view of all that has been said or sung by our correspondents upon the matter, to which undertaking we proceed without further exordium.

The nucleus of the controversy was an abstract of some of the contents of "An Inquiry into the Nature and Causes of Epilepsy, with the Function of the Spleen, &c. By John Jackson, M.R.C.S.L., &c." inserted in the 2nd vol. of The Lancet for 1841-2, p. 476, and from which it appears that the following are Mr. Jackson's views:—The spleen is an organ, the office of which is to propel the portal blood through the portal trunk, branches, plexuses, and hepatic veins, to the inferior cava and auricle, regulating the quantity and force with which that blood is propelled; it is accordingly *de facto* an assistant circulatory organ. Mr. Jackson's opinion is grounded on the following circumstances:—That the *vis a tergo* of the heart's action appears to be inadequate duly to propel the blood through two venous and two capillary systems, and that an additional power is therefore required at the origin of the second venous and capillary system. That the spleen, by its structure, properties, position, and connection with the portal trunk, is eminently adapted for the performance of such a function. That there is another organ (the placenta), the function of which, as well as its texture, is analogous to that of the spleen. That much more venous blood is returned to the heart by the hepatic veins than there is arterial blood supplied to the digestive organs by the cœliac and mesenteric arteries; and that the spleen is relatively much larger in man than in the quadruped; *because a greater power is needed for the propulsion of blood through a venous system, the direction of which is vertical, than through one the direction of which is horizontal* (we have printed this last half sentence in italics because Mr. Jackson subsequently dwells much on the fact as a main support of his argument). There will not be room in this resumé for many words respecting the placenta, the analogy of its function with that of the spleen, &c.; readers disposed to follow the argument at length are, therefore, referred to the original articles, the pages of which will be successively indicated. It must be stated, however, that Mr. Jackson considers the placenta as a *temporary spleen:* thus this organ (like the spleen) receives a thin venous blood,—and he denies that any peculiar change in the blood returned from the fœtus by the umbilical arteries is effected or requires to be effected in the placenta.

In a second paper (Lancet, vol. ii., 1841-2, p. 633,) Mr. Jackson adds to the foregoing statement:—That the veins originating from the capillaries of the stomach, intestines, spleen, pancreas, and gall-bladder, ultimately terminate in the portal vein, where the blood which has passed through one system of capillaries is diluted with the fluids which have been taken into the stomach (all fluids enter the portal vein by the splenic); and that this *diluted venous blood* is then propelled through the divisions and subdivisions of the portal vein, by the contractile and elastic properties of the spleen. The vital contractility of this organ is, says Mr. Jackson, proved by the organ's varying considerably in size at different times in the same animal, in addition to which it possesses an elasticity due to its fibrous capsule and prolongation, as the elasticity of the portal trunk is due to the capsule of Glisson.

Mr. Eagle, in an elaborate paper in the volume quoted above, p. 783, says, "Whatever the product of the spleen, that [illegible] must be conveyed either by the veins or by the absorbent lymphatics," but he infers that it is *not* conveyed by the lymphatics, since there is nothing peculiar in these vessels in the spleen as compared with the same vessels in other organs. He concludes, therefore, that the product of the spleen is conveyed by its veins, or rather *sinus*, for he denies that what we call the splenic vein is in reality a vein, or that it carries true venous blood. From having examined the contents of the veins in the spleen of an ox, Mr. Eagle considers, in fact, that the office of the spleen is *to secrete the colouring matter of the blood;* and he has become confirmed in his conjecture by observing that rabbits from whom he had excised the spleen became after a short time nearly destitute of red blood. Mr. Eagle (at p. 784) quotes

opinions of Hewson, and Tiedemann and Gmelin, in support of his own; and he refers to a paper in THE LANCET for Feb. 7, 1835, by Dr. O'Shaughnessy, who reports that in splenic blood a new principle similar to the colouring matter of the blood, and termed *sub-rubrine*, had been constantly discoverable. Mr. Eagle suggests that its abundance might be owing to "an inordinate secretion or overflow of the product of the spleen." He also quotes the results observed by Schultz in his experiments on the portal and splenic blood (which readers may see in THE LANCET, 1835-6, vol. i., p. 935). Yet though Mr. Eagle attributes to the spleen the office of secreting the colouring particles of the blood, he does not appear to accord to it *per se* the ability to confer on the blood its *red colour*, as the following passage will show:—"I am inclined to believe that the chyle is taken up by the lacteals, becomes globular and albuminous in the intestinal conglobate glands; milky, pale red, or deep *couleur de rose* in the mesenteric veins during digestion; joining the splenic product, becomes dark red in the porta, to prepare the bile in the liver, where it is also rendered susceptible of aeration, emptied by the hepatic veins into the cava to be mixed with the lymph in the right auricle, *thus becoming fibrinous;* aerated in the lungs to form finally arterial blood; ex-aerated and deprived of its superabundant lymph in the cutaneous capillaries, to become again venous, and thus hæmatosis is not one process alone, but a series of processes; it is not in the lacteals alone, nor in the abdominal lymphatic glands alone, or in the mesenteric veins alone, or in the spleen alone, or in the porta alone, or in the liver alone, or even in the lungs, that this vital fluid is prepared; but the formation of blood embraces a series of processes from the stomach to the lacteals, from the lacteals to the lungs."—Pp. 786-7.

In a subsequent communication in the present volume, p. 58, Mr. Eagle announces for the consideration of the profession the proposition *to tie the splenic artery or vein, or both, or the entire excision of the spleen, to prevent marasmus* in the human subject, or to promote the secretion of fat in the human or brute frame; both which physiological effects, as well as the arrest of development of tubercle, he thinks capable of resulting therefrom. To prove that the operation of excision of the spleen is unattended with danger, Mr. Eagle quotes three cases, one of which is related in THE LANCET for May 20, 1837, p. 374. This impunity is, however, impugned, together with some of the physiological deductions of Mr. Eagle, in a letter by an anonymous correspondent in the present volume of THE LANCET, at page 130.

Mr. Stevens, in his demonstration and argument, &c. (ante p. 553), states that under the microscope the spleen during life is seen to consist of a mere congeries of capillary vessels, of a remarkably uniform size, carrying only a single file of blood-corpuscles, and between which there is no nervous parenchyma or extra-vascular tissue; these splenic capillaries seem to be arranged in loops. Mr. Stevens asserts that a physical objection is opposed to the spleen being an assistant to the portal circulation, according to Mr. Jackson's theory, in that the splenic vein is of a weak and distensible texture, and that not being supported like the vena portæ, by the capsule of Glisson, it would (on contractile efforts of the spleen) be inevitably liable to varicose distention. Mr. Stevens admits that the spleen has belonging to it plenty of lymphatics; but he considers that these may be supposed necessary for removing the refuse material during nutrition, and says that the blood received by the spleen from the cæliac axis traverses the capillaries of the organ, "palpably without any deduction" (p. 556), and is delivered to the portal vein without change. Mr. Stevens admits that the secretion of the bile is diminished by the extirpation of the spleen, and the cutting off the addition of the splenic blood from the portal circulation, and that in such cases the animal grows fat, from the retention of carbon in the system. He says, "We observe that the bodies of stout persons generally contain small spleens, and a sluggish secretion of bile, and a consequent retention of carbon in the form of fat; whilst thin persons almost invariably possess large spleens, and constantly an active secretion of bile." He states also, "Since the secretion of bile is palpably diminished when the splenic blood is cut off from the portal circulation, no one can venture to doubt that the residuous venous blood of the chylopoietic viscera, from which the bile is secreted by the liver, must require this vital addition." But he considers the office of the spleen to be simply the intervention of a capillary system, to avoid that unprecedented infringement of the laws of general anatomy—an arterio-venous anastomosis,—which must otherwise have been to set up to carry the necessary arterial blood (for such, he says, the splenic blood is) into the portal circulation.

Mr. Eagle (ante p. 623) objects to Mr. Stevens's paper in so far as it brings forward no analysis of any kind proving that the splenic vein contains arterial blood, a position which Mr. Eagle denies. In this denial he is supported by Mr. Jackson (p. 655), who, however, at the same time considers Mr. Stevens "right in remarking that no change is effected on the blood in its progress through the spleen." Respecting the other points of objection mutually raised between Mr. Jackson, Mr. Eagle, and Mr. Stevens, we had better refer readers to their

several replies and rejoinders in pages 654, 655, 681, and 754, in this volume.

A correspondent who chooses to shelter himself under the cognomen of "Amicus Veritatis," after starting some objections to the theories of both Mr. Eagle and Mr. Jackson in THE LANCET, vol. ii., for 1841-2, p. 878, considers the office of the spleen to be that of fitting the portal fluid for circulation in the liver by abstracting from the blood supplied to it (the spleen) a great, if not the greater, portion of its serous particles, which are taken away by the lymphatics of the organ, so that there may be presented to the liver "a more highly-carbonised pabulum, and one accordingly better adapted to excite the liver's action, as well as more fitted for the elaboration of the bile. The function of the spleen is then of a double nature; its products are of two kinds, and carried away by two sets of vessels; one kind, the highly carbonised product, conveyed to the liver by the splenic vein and porta; and the other product, the serous or watery, taken up by the lymphatics to be poured by the thoracic duct into the subclavian vein." Conformably with this hypothesis, our correspondent says we ought to find that the blood coming to the liver from the spleen has in it a greater proportion of carbon than other venous blood; and he refers to the researches of Schultz, as quoted in THE LANCET of March 12, 1836, p. 935, to prove that such actually appears to be the case. He also quotes Meckel, who bears him out in considering "that the blood is subjected in the spleen to a change which fits it more fully for the formation of the bile."—P. 881.

Mr. Jackson in the present volume, p. 88, raises various objections to the views of "Amicus Veritatis," but as our space contracts, we must refer readers who wish to examine the points in dispute between these two writers, to their somewhat lengthy, though copious, papers.

We have now given as hasty a *coup d'œil* as possible of the published views of most of our correspondents on this subject, at least down to a recent period. We have now to take up several papers which have unavoidably remained unpublished in our hands for some time, and which would, especially since the lapse of that time, have been found obscure in certain of their parts, but for an abstract, such as the above, of the previous controversy. As we stated at the outset, the papers in question are too long for entire insertion; but in the next number of THE LANCET their leading passages will be published, together with an abridgment or summary of what else they contain.

PHYSIOLOGY OF THE SPLEEN.

SIR,—I have hitherto delayed addressing you on the subject of Mr. Stevens's paper in the hope that some gentleman who has practically studied the subject would have answered his erroneous statements. No one having done so to my satisfaction, may I beg the insertion of the following observations.

Without entering into the details of his view, I must object that his theory is wholly dependent on the following unfounded assumption:—"That the interposition of capillaries is the necessary course which nature universally adopts when a communication is required between the arterial and venous systems."

Now, first, as regards the universality of this capillary communication. In the saurian and chelonian reptiles we find two great vessels arising from the ventricle, conveying severally arterial and venous blood, which unite posteriorly to form the descending aorta. Again, in the human fœtus, we have a similar mixture of arterial and venous blood, so that this communication is not opposed to the "natural laws of physiology." In cyanosis, an abnormal condition, the same arrangement obtains.

Secondly. As to the necessity of this capillary communication. Mr. Stevens adduces no argument in proof of this position. The use of a capillary circulation is to bring the blood into close approximation with the minute parts of the tissues, for the purpose of nutrition, secretion, or changes in the blood itself. Now, according to Mr. Stevens' assertion that the blood undergoes no change in the spleen, what end can possibly be achieved by the capillary subdivision of the artery,—what advantage can be obtained thereby?

I cannot stop to consider the details of his theory, except to ask why the spleen possesses lymphatic vessels so exceedingly numerous, so utterly out of proportion to an organ "containing little or no intervening nervous parenchyma or extravascular tissue." What use do they serve?

I have a few words to say on the subject of the placenta; Mr. Stevens's statements on this organ are quite extraordinary. He states that he can prove a direct communication between the maternal and fœtal circulations, and that the "placenta is merely the necessary capillary communication between the uterine arteries and the umbilical vein, and between the umbilical arteries and uterine veins." I cannot suppose Mr. Stevens to be ignorant of the experiments and conclusions of Hervey, Wm. Hunter, Denman, Davis, Rigby, and others, on this point; but he may not be aware of some experiments made by Wrisberg, which, to my mind, are conclusive, and prove to demonstration that no communication whatever exists between the maternal or fœtal circulations, except through the parietes of the vessels. Having observed that the fœtus appeared to contain the usual quantity of blood in a case

where the mother had died of hæmorrhage, he killed some cows big with calf by bleeding, and invariably found the vessels of the calf as full as under other circumstances. In the face of such facts I must confess to having some doubt of the correctness of Mr. Stevens's observations. With many apologies for having trespassed so much upon your columns and the time of your readers in an unprofitable discussion, I am, Sir, your obedient servant,

WM. WAY, M.B., Lond.

Russell-place, Fitzroy-square,
Feb. 21, 1843.

THE SPLEEN.

REMARKS ON THE

THEORIES OF MR. ROSS AND MR. JACKSON.

SIR,—As I observe you are preparing a general *resumé* on the subject of the spleen, I should like to address to you a word or two on several of the questions which have been started; for it is remarkable how many different views may be taken in accounting for any functional process. I will allude for instance to Mr. Ross's theory (which he has founded on the experiment of some German chemist), concerning the affinity between fat and fibrine, namely, that the fibrine of the splenic lymphatics renders soluble the fat brought by the lacteals into the lymphatic duct; and that a large and active spleen, by furnishing that fibrine, thus prevents the deposit of this fat in the corporeal tissues. This is probably better explained by Mr. Stevens's original position that a large spleen produces an active secretion of bile, and that the *soda* of the *bile* (instead of the fibrine of the splenic lymphatics) renders the fat soluble; and that, accordingly, with a small spleen there is a less active secretion of bile, and a greater retention of hydro-carbon in the blood than can be consumed in the tissues, and expired as carbonic acid and water, the hydro-carbon being therefore retained in the form of fat. Mr. Ross says that branches from the hepatic artery pass *directly* to branches of the vena porta. Mr. Stevens has not denied their union in the capillary form; but one may explore in vain to find any direct union of their larger branches. That the hepatic artery is solely for the purpose of nourishing the tissue of the liver, is pretty well borne out by the ramusculus cysticus alone.

The terms of cellular and cavernous, as applied by Mr. Jackson and others to the spleen, are not warranted by dissection, unless the following arrangement be thus mistaken. The splenic artery before entering the organ suddenly breaks up into several short branches, which, in the short capacity of the organ, are again broken up into capillaries. The larger branches (arterial and venous) being very short, may be taken for cells. But this arrangement is only an economy of room, accounted for by the absence of extra-vascular tissue in the spleen and placenta; for the vessels commence breaking up into branches even before entering these organs, in order the sooner to reach the capillary form. Mr. Jackson implies an active contractility in the fibrous tunic of the spleen; I can admit a passive elasticity only, such as exists in certain other fibrous tissues, as the ligamentum nuchæ of grazing animals. The reason why it is difficult to inject the placenta so as to show connection of its capillaries, appears to be because the blood-globules have become packed in these vessels, and because the injections commonly used are coloured not by colouring matter in solution, but by levigated powders, the particles of which are as large as the blood-globules themselves. But if a stain-tinted size-injection be used very gently, while the placenta is just fresh, the proper course of the vessels will easily be detected. The only objection to this preparation is that it will not keep. I remain, Sir, your obedient servant,

CAPILLARIUS.

February 18, 1843.

RENAL DISEASE SIMULATING CALCULUS IN THE BLADDER.

M. SEGALAS reports that he was called to attend on a child, from four to five years of age, which often placed its hands on the genitals, and experienced frequent desire to pass urine, which action, however, was always performed with pain. The urine was tolerably abundant, and yielded a muco-purulent deposit. The acuteness of the various symptoms had been considerably increased during some recent attacks of roseola and bronchitis. These symptoms apparently indicated the presence of a calculus in the bladder; M. Segalas, however, doubted that one really existed, having never met with this disease in any children but those of parents in a much lower sphere of life than those of this patient; and a careful examination proved that no urinary calculus in reality existed. Some time afterwards the child died of a cerebral affection, and the body was opened. The bladder, as well as the urethra was found perfectly healthy and free from urinary deposit; but the left ureter was much dilated, and the pelvis of the corresponding kidney three or four times larger than that of the opposite side. The calices also were enlarged, and the cortical substance of the kidney was inflamed at several points. The right kidney was healthy.—*Gaz. des Hop.*, Jan. 7.

THEORY OF MENSTRUATION.

To the Editor of THE LANCET.

DEAR SIR,—The accompanying practical observations and judicious comments of my acute friend, Mr. Girdwood, relate to a subject so important in reference both to physiology and to medical jurisprudence, and contain so just and candid a tribute to the ingenious though neglected researches of a late distinguished practitioner of this Metropolis, that I feel assured you will, with me, consider them as worthy of being submitted to the profession through the medium of your valuable Journal.

This communication has been drawn up by Mr. Girdwood, at my solicitation, and the beautiful theory established by Dr. Power, and here so ably illustrated, is in harmony with all the best-established facts in comparative anatomy, though at variance with the crude and contradictory opinions so generally entertained on this subject.

I have only to add that the cases and views here detailed by Mr. Girdwood, were fully announced to me by that gentleman, and repeatedly discussed between us, as early as 1836, when he was not aware of the extent to which the important results of his experience had been anticipated by previous observers, and at which period I solicited him to direct the attention of physiologists to these most interesting facts. I remain, dear Sir, your obedient servant,

ROBERT E, GRANT.

Seymour-place North, Euston-square,
Feb. 20, 1843.

My dear Sir,—I at length comply with your urgent request, and place before you my inquiries on the subject so often mentioned to you in 1836, viz., the exact correspondence in the number of cicatrices found on the ovaries of females with the number of times they have menstruated, and on the probable dependance of that periodical discharge on the descent of ovules through the Fallopian tubes; and, in illustration of my views, I now send for your inspection the ovaries of a young unmarried lady who died a few days ago, and who was sixteen years and ten months old at the time of her decease. Ten months ago her fatal malady commenced, and at the same time she ceased to menstruate. The catamenia first appeared when between twelve and thirteen; they were regular in their appearance during the three years they continued to occur. Thus, she had about six and thirty periodic returns. On examining these ovaries you will observe that the surface presents several indentations, some isolated, some grouped together and incident upon each other. These indentations, or small cicatrices, are about the size of a mustard-seed; they are oblong in form. Those in groups have the oblong shape less evident than those which are isolated, and are altogether not so well defined; but still, without much difficulty, the eye can detect spot after spot in each group, and single out almost every constituent indentation. In all I think from thirty-two to thirty-four of these marks may be reckoned,—about eighteen in one and about sixteen in the other ovary.

To explain the causes of these appearances let us place a series of facts in consecutive relation to each other.

The ovary of a female who has never menstruated is a soft pulpy mass, oval in shape, with a regularly defined outline, and possesses a smooth, polished, glistening surface.

In a female who has menstruated e. g. for ten years, and whose second lustrum of menstruation has thus passed, the external characters of the ovary have much changed. The soft, glistening, polished surface is now seamed and scarred throughout; its regular oval form no longer exists; deep corrugations have destroyed it; scars, some still isolated, appear, but a mass of cicatrices, irregular and ill-defined, generally marks the surface.

The ovary of a female approaching anility, who has ceased to indicate, by the usual periodic secretions, her capability for fecundation, has lost entirely its regular rotundity and plumpness, and instead of a soft, pulpy, glistening substance, it is a grisly, withered, corrugated mass.

Examples, in illustration of these facts, are easily produced. I wave, however, all others excepting those I am desirous of placing before you as indicating, in my opinion, that those indentations on the surface of the ovary are correlative in number with the periods of menstruation, and that the menstrual discharge is a secretion excited by the presence of an ovum in the ovary at its full period of development, and in the act of disengagement from it and transmission through the Fallopian tube.

Before relating these, let me observe that small vesicular ovules exist in great numbers in the stroma or parenchyma of the ovary; that they may be seen there in different stages of development; that some, far in advance of others, are so increased in size, that even in the ovary of a six months' fœtus they are evident without the use of a lens.* As the female approaches puberty, some one of the ovules becomes more and more developed, approaches the surface, and, as in the instance I am now going to relate, it occasions a transparency of some degree over this small Graafian vesicle.

In June, 1834, a whole family were ill of scarlet fever, under the care of Mr. Richardson, of Bayswater, and myself. A girl, be-

* These were distinctly seen by me in a six months' fœtus of a pregnant woman who drowned herself in Paddington canal in 1834.

ween eight and nine years, succumbed to the disease. In her the ovaries presented the usual polished, glistening surface of those of a female that had not menstruated. A spot, however, on the left ovary was distinctly perceptible, and arrested attention; it was to some extent transparent, slightly elevated, and surrounded by a net-work of injected capillaries. On cutting open this ovary the transparency and elevation were found to be owing to one of these vesicles, very much enlarged, and indeed now projecting immediately under the peritoneum.

Had this girl lived and reached puberty, what would have happened in respect to this vesicle? I have no doubt that it would have escaped from the ovary, passed through the Fallopian tube and uterus, and that this phenomenon would have been coexistent with the first period of menstruation; and had she died immediately after or during the act of this first menstrual secretion, appearances similar to what I have now to notice would undoubtedly have been found.

In December, 1832, Ann Davies died of scarlet fever, under the care of Mr. Webster, surgeon, of Connaught-terrace, and myself. The menstrual flux made its appearance *six hours before dissolution*. On the left ovary there was an oval cavity, large enough to admit a tare, and no doubt left by the recent escape of an ovule; its margin was unequal and flocculur, and, as well as the walls throughout, of a deep-red colour. This female had menstruated for years, and the appearances which I consider as the usual indications of frequent menstruation, as already described, were evident on the surface of the ovaries.

The ovaries and the uterus of this female were carefully removed from the body, and the day after our examination were presented to Dr. Robert Lee, to whom I repeated (what I had previously communicated to him) my views on this and similar appearances, as detailed in this communication, but I could not then induce that distinguished pathologist to adopt my views.*

In my case-book I find, under the date of May 23, 1835, the following memoranda:—"Emma Bull died yesterday, dropsical, the result of diseased heart and lungs. Two years ago, during an attack of asthma, after much pain in the pelvis, she menstruated, this being the first and only time she had ever had that secretion. I opened the body this morning. The ovaries were plump and rather larger than usual, soft to the touch, and glistening. There were here and there diaphanous specks, caused by the absorption or thinness of the peritoneal coat, and the approach to the surface of the Graafian vesicles. There was no mark or scar whatever on the right ovary; but on the left, about the centre of the posterior surface, there existed a reddish part, about the size of a mustard-seed, which had quite the appearance of an ulceration skinned over. The uterus and its appendages are in the possession of Mr. Webster."

During the same year a young woman also died under my care. She had menstruated three times. The surface of one ovary presented two cicatrices; that of the other, one similar to that described in the last case.

In January, 1834, Jane Cumming, eighteen years of age, died of consumption. She had menstruated only six times. Mr. Webster, who was aware of my views regarding the exciting cause of menstruation, and of my anxiety to examine the ovaries, allowed me to be present at the autopsy. We could readily detect five depressions or cicatrices, three on one, two on the other ovary; of a sixth we were doubtful.

In August, 1838, I opened the body of Miss G., who had not completed her sixteenth year, and who had been regular for two years previous to her sudden death. In her I found about twenty-two of the usual marks on the ovaries. All these appearances, as they occurred in these different cases, were brought under the notice of my friend Dr. Robert Lee, but still without being able to convince him that the views I entertained respecting their cause, as above explained, were correct.*

I might extend my communication by cases of a character similar to those I have related, and which were accompanied by similar results. Enough, however, I hope has been stated to establish an opinion expressed to you in the session of 1836-37,

* In THE LANCET of June 29, 1839, is given the account of a meeting of the Medico-Chirurgical Society, at which a paper by Dr. Lee was read, on the corpus luteum, and when Mr. Solly mentioned that in a young lady who had died shortly after her menstrual period, and of whose chastity there could be no doubt, there existed appearances of a nature similar to those in this case, and which were perfectly in accordance with the views I have developed. The expression used is that "an enormous corpus luteum was discovered."

* Indeed, I cannot find that in his frequent published communications Dr. Lee takes any notice of these cicatrices until last year, when I perceive in his lectures he incidentally alludes to them. Mentioning the case Sir A. Cooper sent him, and those I showed him, he adds, that he "was not then aware that cicatrices are never present on the surface of the ovaria before menstruation has commenced." This is quite true, for although I had seen cases often enough to satisfy myself of the truth of my views, the cases related in this letter were the first of so decided a character, which it was in my power to offer for the conviction of others.

when I, as now, attended your lectures, and enjoyed an hour's emancipation daily from my professional fatigues.

Well do I recollect your scepticism on this matter, and of my comparison of a woman to a domestic hen, another biped; but the woman, instead of, like the bird, depositing her egg daily for some indefinite period, only takes more time to mature it, and requires a more tedious and complicated process to void it only once a month.

What? I hear it asked, Are we to credit in our own species the fact of a female passing eggs every few weeks for twenty or thirty years? The statements I have made above, however, are of facts, and such as are capable of easy corroboration. Nor, indeed, is there anything wonderful in a female passing periodically these numerous eggs, as asserted. Let her be supposed to continue for twenty or thirty years in this fecundative condition, with her ovaries performing regularly their functions, there would have been, even then, only from two hundred and forty to three hundred and sixty ova passed; and, tracing the state of these organs from their soft pulpy condition previous to puberty, through the regular changes they undergo as the woman advances in years, up to the withered, shrivelled, corrugated form they present when anility arrives,—is it too much to conceive that each ovary may have had from one hundred and twenty to one hundred and eighty times its surface opened for the escape of so many ovules?

At all events I repeat the fact, now attested by my numerous observations, that there exists a most close and remarkable relation between the number of cicatrices on the surface of the ovaries and the number of times a female has menstruated.

The question which, in this communication, I have attempted to solve, has been of late much obscured by the circumstance of those who have attempted its solution limiting their attention to the development of the human ovum alone. From the perfect innocence of comparative anatomy enjoyed by most modern writers on the subject, and by their professional avocations, guided to appearances in the human uterus and ovaries alone, their inquiries have been unfortunately limited to the investigation of human generation only, and thus, by being confined to this limited sphere of observation, and from their position taking, as it were, a monocular view of the matter, they have been deprived of all the illustrations and analogies in other classes of animals, of all the advantages to be derived from the wider contemplation of animated nature, and of the marvellous unity of design in the generative process evident throughout the whole series of animated life.

The eyes of physiologists have been directed to find out the mysterious moment when, during impregnation, or soon after that circumstance, the Fallopian tube has grasped the ovary, and drawn into its canal the impregnated ovum. When appearances have favoured the idea of impregnation, the cavity on the surface of the ovary was, of course, regarded as a *true* corpus luteum; but, on the other hand, when there were no grounds for inferring impregnation, such an appearance was called a *false* corpus luteum.

The barbarous nomenclature in use by physiologists, the same expression being used by different authors in different meanings, has been another great means of obscuring this subject. "*Graafian vesicles*," "*ova*," are used as if they were different things. Then we have "*the corpus luteum*," "*the true corpus luteum*," "*the false corpus luteum*,"—the one the result of impregnation, the other something as yet not satisfactorily accounted for.

I may as well define what these expressions mean, and thus get over the *pons asinorum* of the subject.

Ovum, corpuscle, Graafian vesicle, are different names applied to those bodies existing in great numbers in the ovary of all females, not only in woman, but in the ovaries of all females where the sexes are apart,—terms, all of them expressive of those oval bodies found so numerous, and periodically, by all females of all animals, discharged from the stroma of the ovary at maturity, and that maturation effected by the *female* herself, without any influence having been exercised by the male; in fact, an ovum, as here defined, containing a macula germinativa within a vesicula germinativa, and this surrounded by the yolk or nutritive material, adapted for the future development of an embryo, is the elaboration, as far as female influence extends, in preparation for the future being, and waiting only for the influence of the *male* as a stimulus for its development.

The corpus luteum I define as the nidus in the stroma of the ovary from whence has escaped an ovum. Whether that ovum has been impregnated or not impregnated cannot be manifested by any textural difference in the structure of the nidus itself. The appearances must be, in both cases, identical in nature, or if they differ in any degree the difference must be simply one of intensity. An ovule, impregnated, may in the stroma of the ovary become more enlarged, more matured, and by having received the male influence, more developed, and thus occasion a vacant space to be produced on the periphery of the ovary, only larger than would be produced by a non-impregnated ovule.

It is true, that when an ovum has arrived at maturity it may not escape from the ovary; it may there become atrophied, and be absorbed, and may then give rise to an appearance somewhat resembling a corpus luteum.

The cicatrix is merely the permanent in

dication of the previous escape of an ovum, and bears the same relation to the corpus luteum as any other cicatrix would to a wound with tumid edges.

Thus I conclude the results of my own personal experience, and the reflections resulting from them, and thus fulfil the promise which you long since exacted from me. And here my communication would have had an end, had it not been that, since I have communicated these facts and opinions to paper, you have directed my attention to the work of Dr. Power on the subject I have been attempting to illustrate.

I consider myself peculiarly fortunate in having had, by you, so opportunely my attention thus directed; as, although the perusal of this elegantly composed and highly philosophical work deprives me, on the one hand, of any merit I might have claimed, on the score of originality, in the views I have long been attempting to elucidate; yet, on the other hand, I am saved the mortification I must have experienced had I, in ignorance of a prior claim to them, developed them as my own.

I have still left me the grateful duty of doing homage to a man, whose writings seem strangely to have fallen into oblivion, and rejoice in being the means of bringing again to the notice of physiologists the claims of a most talented author, and restoring to him that high station as a discoverer which his researches, founded on the comprehensive views and highly philosophical generalisations developed in his truly classical essay on this subject, entitle him to occupy.

I feel the more pleasure in paying him this candid tribute of respect from finding how nearly my own personal experience has led me to arrive at conclusions similar to those entertained by him. Ignorant of his essay, I had advanced, step by step, to results almost identical with those which Dr. Power, founded by him on much personal experience, as well as extensive and well-digested researches in the writings of ancient as well as modern authors, had so ably and fully illustrated more than twenty years since.

In proof of the neglect in which this admirable work has been placed so long, I may observe that Gendrin, in 1839,* publishes, as novel, views identical with those of Dr. Power; and Dr. Willis† alludes to Dr. Power, as having merely loosely mooted the question as "an hypothesis," instead of establishing it as a theory, in an essay, which is a model of inductive reasoning, founded on much personal experience, and illustrated by extensive inquiry. In illustration of the oversight of some, even of our most distinguished investigators, with regard to the fundamental points so long since established by this author, I may cite no less an authority than our distinguished countryman, Dr. Robert Lee, who, in the "Cyclopædia of Medicine," published in 1834, article Diseases of the Ovaria, referring to the investigations of Dr. Power,* as detailed in his work, states them, as does Dr. Willis, to be merely "an hypothesis." Some unaccountable misconception of terms seems to have occurred to Dr. Lee, for whilst combatting Dr. Power's statement, that an ovum passes during menstruation, he singularly enough makes use of the following remarks:—

"That an ovum, by which is usually meant *an embryo developed in membranes*, does not pass from the ovarium during menstruation, is evident from the fact that *an ovum is never formed but as a consequence of impregnation*, and that conception does not take place at the menstrual period."

Dr. Power's philosophic mind was not guilty of the supposition that to constitute an ovum it was necessary an embryo should be within it. The term with him is *generic*, applicable to the ovum *unimpregnated*, as well as to the ovum *impregnated*.

I am myself induced to quote from Dr. Power, and select a passage, page 15, where he says, "that in women who have never been impregnated, corpora lutea and cavities, which have been supposed previously to have contained ova, have been detected; whence it may be inferred that in them not only the formation but the extrusion of ova is accomplished without the influence of the male."

It is evident from the above passage that Dr. P. uses the word *ovum* not as confined to express its condition after impregnation, when it contains the embryo, and thus to the limited sense in which it is applied by Dr. Lee, but that it is used by him in the sense in which it is generally received, indicated by its derivation, and rendering it applicable to the product of the ovaries of females in other classes of animals, as well as to those in mammalia,—even to those instances in which extrusion takes place previous to impregnation, as in most fishes and amphibia,—as well as to those instances, as in the human

* Traite Philosophique de Medicine Pratique, 1839.

† Translation of Wagner's Elements of Physiology, London, 1841, Part I., p. 69, in the note.

* Dr. Robert Lee refers to "An Essay on the Periodic Discharge of the Human Female," London, 1832. I have not been able to meet with this essay by itself, but refer to it as published in "Essays on the Female Economy," by Dr. Power, in 1821. Indeed, I understand from the author himself, that no other edition than the one of 1821 is he aware of ever having been published.

subject and all other mammalia, where impregnation is invariably internal.

Nor is Dr. Power's idea of a corpus luteum confined to the appearances left by an ovum *after impregnation* only. With him there is no distinction between true and false corpora lutea. He applies the expression to describe the appearances existing after the escape of an ovum *unimpregnated* as well as *impregnated*. In this position he again widely differs from the most classical authors of the present period. Dr. Burns, Dr. Ramsbotham, Dr. Robert Lee; the writers, in fact, whose works are in the hands of students at the present time, all agree in considering the expression as applicable, truly, only to the phenomena appearing after impregnation.

I perceive my acute young friend, Dr. Robert Paterson, of Leith, adopts, to the fullest extent, the faith of the hypothesis of Dr. Robert Lee.* He has, however, incurred, in no measured terms, the severe animadversions of that learned lecturer, not for his scepticism in the faith of corpora lutea existing only after impregnation, but for the application of what he considered to be the true principles of that faith to explain appearances in the ovary of a female that came under his observation. This female had been long married, but effete. Some short time previous to her death her matrimonial fidelity was suspected. In one of the ovaries of this woman Dr. Paterson discovered appearances that not only he considered, but that Dr. Allan Thomson and Dr. John Reed also considered, as a true corpus luteum, and, therefore, that this female had experienced impregnation shortly previous to death.

In this controversy Dr. Robert Lee disputes the opinions of these physiologists, commencing his opposition on grounds that, in the first place, in the uterus or Fallopian tubes "no ovum, nor any vestige of an ovum, could be detected," and summing up his strictures by the following passage, "Nobody has come forward with a testimonial in behalf of this corpus luteum, or asserted that he saw two membranes around the coagulum of blood, and a layer of coagulable lymph in the centre, filled with a reddish grey-coloured fibrinous matter. The character of this corpus luteum is, in truth, gone for ever—far more completely blasted than the reputation of the poor lamp-lighter's murdered wife."

These differences of opinion among men of such reputation as physiologists are to be regretted. They might never recur were terms of a more precise and philosophic character, to be used instead of the expressions "true" and "false corpora lutea," now so generally in use.*

Most probably, this female had lately menstruated, and the appearances observed, and so minutely described by Dr. Paterson,† were the result of the escape of an ovum *unimpregnated*, and of course the same in structural character with those appearances that are insisted upon by Dr. R. Lee as indicative of the escape of an ovum *impregnated*.

But although Dr. Lee in the controversy respecting this female lays much stress on the non-detection of an ovum, yet it would seem he does not always rely on it, for he does not mention its detection, nor, indeed, could any one well expect to find it in the instance where he relates that he found "a perfect corpus luteum in the ovary of a woman who died from inflammation of the uterus soon after the expulsion of a mass of serous cysts or hydatids, as they are called, from its cavity."‡ And it may be some consolation to those whose minds are perplexed between true and false corpora lutea, and with the difficulty of distinguishing the difference—a difference some, too, may be inclined to consider to be only a difference without a distinction—to find that Dr. R. Lee, after the enumeration of all the minute points in which he considers them to differ, sums up his remarks by candidly stating that although "corpora lutea, such as have been now described, may be regarded as furnishing the most unequivocal proof of impregnation," yet, he adds, that "*it is necessary to know that conception may have taken place, and that the corpus luteum may be so small and so imperfectly formed as to be in a great degree destitute of its genuine and distinctive characters.*"

I willingly pass, however, from this vague verbal controversy, so humiliating to science, and quote this beautiful passage from Dr. Power:—

"If the prolific stimulus is denied, the ovarian action may be insufficient for the

* For Dr. R. Lee's opinions vide lectures in the "Medical Gazette" of last year; also his paper on the Corpus Luteum, "Med. Chirurg. Transactions," vol. xxii. In a discussion following the reading of this paper, LANCET, June 29, 1839, in a remark in reply to Dr. Bright, who expressed his anxiety to have the society enlightened with regard to the differences of appearance between a true and false corpus luteum, Dr. R. Lee is reported to have said that the false were either larger or smaller than the true, and that the effusion was around the two coats of the corpus luteum.

* This controversy begins in Dr. R. Lee's lecture, as published in the "Medical Gazette" of 1842, No. 7; is carried on in Nos. 11, 12, and 16, and ends in 17, where "Scrutator" has the last word.

† In a series of essays in the "Edinburgh Medical and Surgical Journal," Nos. 142, 145, and 147.

‡ Med. Gazette for 1842, p. 195.

necessary detachment of the ovum; in this case it is probable that, as a useless structure, it perishes, loses its connection with the uterine system, becomes an extraneous body, and is, by the process of absorption, removed from the ovarium; or, in some cases, where the constitutional or local actions of the uterine system are excessive, the degree of ovarian action may be sufficient to produce the detachment of the ovum. The corpora lutea found in the virgin ovaria may be regarded as the cicatrices remaining from the ulceration attendant on this detachment; and the influence of the excessive ovarian action, it is presumed, will be sufficient to account for the formation of the membranous and decidua-like skins in dysmenorrhœa."

Beautiful and eloquent as is this passage, and expressed with the characteristic modesty of the author, it is one only of the many happily-illustrated views of Dr. Power with which I am inclined to differ. From the context of this quotation I infer that the withering of the ovum in the ovary, in the opinion of this author, is the more general rule, not the exception. My experience would induce me to reverse the position, and consider the extrusion of the ovule during menstruation as the general rule, its remanet and atrophy there as the exception.

From this valuable essay I might quote sentence after sentence illustrative of how little we have to add at the present time to the views so satisfactorily developed twenty years since. To enable us to perfect a clear, unbroken, and comprehensive view of all the interesting and perplexing phenomena of the functions of the ovary and uterus in respect to generation, we have only to take Dr. Power's essay—the first essay in Dr. Power's work entitled "Essays on the Female Economy"—as our text-book.

In fact, to republish it entire would be the best means we could adopt to rescue the subject from the abject condition to which the constant employment of ill-defined terms and worse-examined cases have reduced it, and place the phenomena of human generation once more in harmony with those of the rest of organised beings, and add the only link wanting to establish uniformity in the generative function throughout the whole system of animated nature. I am, my dear Sir, your's very sincerely,

G. F. Girdwood.

Paddington, Jan. 27, 1843.

To Professor Grant, University College.

The spinal accessory nerve, arising from the posterior part of the spinal cord, seems to be destined to move the muscles which it supplies, viz., the sterno-mastoid and trapezius, in obedience to sensation, but not to volition.—*Prof. Alison.*

MISCELLANEOUS CONTRIBUTIONS TO PATHOLOGY AND THERAPEUTICS,

By James Richard Smyth, M.D., London.

IMPOTENCE AND STERILITY.

The following case we consider a fair, and in a practical point of view, an instructive example of impotence, which depended for years upon causes chiefly constitutional,—causes which consisted, we conceive, more in an imperfect development and ill diathesis of the system, with consequent weak functional habits, than in the presence of any distinct disorders, as in the case of Mr. B., last treated of (page 532).

Mr. J. G. S., aged 28, of a sluggish leucophlegmatic constitution, and of a melancholic temperament; no obvious emaciation, but vascular and muscular systems generally atonic and soft; no appreciable lesion of respiration or of circulation, but pulse rather languid and animal temperature low; countenance pale and of rather sad expression; pupils somewhat dilated, and irides decolourised; alvine and urinary excretions stated to be of a healthy appearance; appetite inordinate; testes of the ordinary size, but softer than natural; epididymi contracted and empty; scrotum and penis lax and elongated. He states that he is at present, and that he has been so for years, subject occasionally to nocturnal seminal emissions of a thin watery character, and that he has never been able to perform the act of sexual intercourse, although he has frequently attempted it, and also that for three or four years after he had arrived at the age of twenty-one he practised the sin of the "trespass of Onan." (Genesis, c. xxxviii., v. 9.) But we cannot do better here than give the patient's own graphic account of his case from an early period:—

"Sir,—According to promise I proceed to lay before you a history of my case, and in so doing I shall trace back further, perhaps, than is necessary, but in this respect you will be the best able to judge. In the first place I would inform you that in early youth, viz., from ten years of age until sixteen, as well as I can recollect, I laboured under a scrofulous state of my constitution; was the subject of venereal desires and nocturnal emissions at fifteen, from which time my health seemed gradually to improve, until the age of eighteen, when I had an attack of inflammatory fever, which left me greatly debilitated. At twenty-one I seemed nearly recovered from my attack of fever, and my sexual powers appeared to revive; but about that time, while washing myself, I became accidentally acquainted with Onanism, which folly I practised more or less until the age of twenty-five, when I became better acquainted with the laws of my physical constitution, and I have no doubt I should

have escaped the evils which have resulted from that vice had I been better informed, and had had facilities for the natural exercise of my generative functions. Between this age, twenty-five, and my application to you, I made several attempts to have connection with ladies of the town, but never could succeed in obtaining a perfect erection (at the time of attempting, in particular) before emission took place, which I attribute partly to the fear I laboured under of contracting disease, and partly to disgust, the affection having been absent as well as the physical ability. And I must also state, that during the greater part of the latter period I have mentioned I was frequently thrown into the company of a young female, whom I used to romp with and take upon my knee, and on many occasions these, of themselves, have caused emissions, and even conversing with her but for a few moments has frequently caused a slight discharge of albuminous fluid from the urethra for two or three hours. I will now describe my feelings and condition during the last eighteen months, or two years, which, with more or less exactness, will apply to my state for the last ten years of my life. In the first place I have been troubled with frequent lowness of spirits; pains along the spine; irritation of the bladder; fainting sensation before dinner; bashfulness; timidity; swelling of the hands and feet with very little exertion; generally costiveness, but sometimes relaxation of the bowels; copious perspiration from a little extra exertion; soon feel chilled in cold weather; appetite generally good and sometimes voracious; often after meals an unpleasant feeling of fulness, with headach and drowsiness; sometimes troubled with painful though imperfect erections, accompanied with considerable tenderness of the extremity of the penis. Frequently the thoughts of these things would cause me great mental uneasiness; again serenity of mind would return on some object being presented to it which caused its exercise. Then, again, I used to feel that if I could but exercise the generative functions I should be as happy as nature would permit. Thus was I tossed to and fro by conflicting feelings, when, about eight or nine months before I applied to you, further evils were added to the foregoing long catalogue, one of which engaged my attention more than all others, viz., dimness of sight, with great humidity of the eyes. I now began to think more seriously than ever of my situation, and to fear that I was doomed to perpetual celibacy, and the subject daily engaged more and more of my attention, in fact, it haunted me the first thing in the morning and the last at night, which had a constant depressing influence on my spirits that led me to entertain serious apprehensions as to the effect it might ultimately have upon my mind, when, from the circumstances I mentioned, I was induced to apply to you, since which time, through your kind attention and advice, many of those symptoms are removed, and all considerably alleviated. This statement, Sir, embraces, I think, the principal features of my case; I therefore beg to subscribe myself your much obliged and obedient servant, J. G. S."—Wells-street, Oxford-street, January 19th, 1842.*

This patient came under our care on the 4th of September, 1841. The treatment of his case was commenced by the administration, twice a day, during ten days or a fortnight, of resinous warm aperients in the form of pill, with a bitter stomachic infusion. By such medicines the functions of the alimentary canal were quickened and strengthened, and the character of the alvine secretions and excretions improved. On the 17th of the same month we prescribed, and he began to take five grains of Plummer's pill every morning, with a chalybeate in infusion of quassia twice a day. We also prescribed an increased quantity of animal food, with some good beer; abstinence from wine and all spirituous stimuli; moderate exercise and regular and early hours in going to rest and getting up; abstraction of the mind as much as possible from all amorous subjects, and the exercising of it, on every occasion of leisure from business, in the study of some interesting work of practical science. This mode of treatment, medicinal and dietetical, was pursued for upwards of three months with the most satisfactory results. Every time, indeed, we saw the patient his symptoms and condition in all respects presented additional evidences of improvement. The depression of spirits, the fainting sensation ante prandium, and the spinal and vesical irritations, gradually became extinct. The general debility of the circulation and of the vascular system, as exhibited in the swelling of the hands and feet, and in the copious perspiration from little exertion, and in the morbid chilliness from cold, were much relieved. Only a trace, in fact, of these symptoms of circulatory lesion remained when he left off consulting us. The headach, drowsiness, and sense of gastric distention after meals, the irregularity of the appetite and action of the alimentary canal, and other anomalous dyspeptic symptoms, all vanished. The irregular seminal emissions and the urethral discharge, were completely arrested, and the perfect erectile power of the penis restored. The wateriness of the conjunctiva, and consequent dimness of sight, were removed. The constitution and entire organic economy, in short, of this individual seemed altered and to a great extent renewed—regifted, as it were, with fresh life and spirit of fecunda-

* We had permission from this patient to publish his case, or we would not have done so.

tion, like the ever-fruitful earth on the return of spring. From the date above mentioned, namely, the 17th of September, 1841, until the 4th of January, 1842, the compound calomel pill of Plummer, with an aloetic and rhubarb pill, were used regularly once and sometimes twice a day. During the last month of their administration the gums were slightly turgid and more vascular-looking, as were also, let us observe, the glans penis; which latter part, more particularly its corona, would appear to evince the constitutional action of mercury as early or perhaps earlier, than any other part of the system, before the gums or the inner surface of the lips, or the pulmonary or cutaneous exhalations.

In certain cachectic and other chronic morbid states of the system, both in the male and female, when (after some purgation) it is an alterative medicine that promises to be of further benefit, there is none, according to our experience, so appropriate and efficient as the compound calomel pill. For combating an acute inflammatory affection of any important organ or membrane, with its consequent effusion and obstruction, —such as, for example, an attack of acute pneumonia, of acute hepatitis, or of acute inflammation of the pericardium or peritoneum, after moderate blood-letting, with other necessary antiphlogistic and preparatory measures have been had recourse to,—calomel to gentle ptyalism is without doubt the remedy chiefly to be relied on; and in some chronic inflammatory disorders, and chronic local disease and obstruction, when time is at the service of the physician, and it is an important principle, too, in judicious treatment to avail himself of it to act upon the economy and alter and remove diseased action and structure *slowly*,* blue-pill, perhaps, taking all circumstances into practical computation, is the best agent that could be employed. But in cases similar to the one we are now commenting upon—in cases, observe, in which the pathology is not a thing of morbid relievo, positive and substantial, but more a condition altogether of degree—of physiological imperfection, the proximate causes of the affections consisting, for the most part, in weak and languid circulation generally, and tardy and defective performance [of all the vital functions,—Plummer's pill, as an alterative, rightly timed and apportioned, is without an equal. It seems to us to enliven and excite, in a peculiarly salutary manner, all the nutritive actions of the economy, and also to leave upon these, after its administration has been discontinued, a somewhat persistent impression of its virtue. No simple or compound medicinal substance that we are acquainted with possesses the power, to such a degree, of developing animal heat generally throughout the system, without the weakening accompaniment of perspiratory moisture; on which account, perhaps, it is that we have found it an exquisite stomachic, tonic, and deobstruent, in cases of dyspepsia, of general cachectic debility, and of amenorrhœa, in persons of cold phlegmatic temperaments. Sometimes alone, and occasionally in more or less quantity in union with myrrh and aloes (as in the pill of aloes with myrrh of the Pharmacopœia), we have found it successful in the cure of amenorrhœa after, chalybeates and a variety of other emmenagogues had been tried in vain; we have also witnessed its good effects in the cold species of chronic rheumatism. It is unquestionably, we think, the mildest and most delicate alterative we have, and would seem to hold a position in the class of medicines of such nature analogous to that which Mr. Travers has, with nice discrimination, allotted to henbane in the class of sedatives. "The virtues," observes this eminent and experienced writer, "of other sedatives are, in my observation, pretty correctly appreciated. There is, however, one deserving of particular mention, both as an adjunct to opium and the best substitute when this is inadmissible." And to this the following description of the medicinal properties of henbane appears in a note which is well deserving of a place in the text of any medical treatise:—"In the ruffled states of the system generally, but especially in the over-active state of the vascular system, there is a charm in the operation of henbane altogether peculiar. It is feeble as an anodyne, feebler as a soporific; but not 'poppy, nor mandragora,' soothe and still so unexceptionably as henbane."*

As a matter of some practical usefulness, let us again direct attention to a point of generative pathology which, in our first paper upon impotence and sterility, we took occasion to notice, and of which the present case, as we see, affords an exemplification, namely, the immediate connection that would appear to exist between the thin and imperfectly-formed seminal fluid and its irregular and premature emission. There is a sort of chemical association in the qualities

* *Tuto, celeriter, et jucunde.* The therapeutic precept of the Roman physician, Asclepiades, is excellent, but only properly applicable to the treatment of acute diseases, in many cases of which, be it observed, the natural rapid progress of their pathology to fatal termination within a determinate brief space of time, is the greatest difficulty which the practitioner has to contend with. To arrest their progress then, and thereby merely gain time for the administration of remedies, and the development of their beneficial effects, is frequently three-fourths of the way towards cure.

* Travers on Constitutional Irritation, page 479.

and conditions of our fluids, and a certain concomitance and co-operation in the actions and functions of our economy, morbid as well as healthy; so that, in the matter of any analytic minute diagnosis, where one morbid quality, or morbid action or phenomenon, is distinctly observable, the presence of another or of others may, generally speaking, with every confidence be inferred. This observation will apply to the unhealthy character of the testicular secretion and to the excessive irritability of the vesiculæ seminales and of their assistant ejaculatory muscles of the urethra and perineum; to the irregular seminal emissions, and the imperfect erectile function of the penis,—generative lesions, in the foregoing described case, associated and all dependent upon one common cause, i. e. great constitutional debility. Sir Astley Cooper, in some remarks to be found in his lectures on this subject, has given, as two distinct causes of impotence in the male, first, excessive irritability of the vesiculæ seminales, and secondly, nocturnal emissions. But with all deference, in the fullest sense of the word, due to such authority, we cannot coincide in the etiological distinction there drawn, as morbid irritability of the seminal vesicles and disordered irregular emissions are obviously one and the same sexual lesion, the disturbed emissions being only the symptomatic manifestation of the irritable condition of the receptacles whence they emanate. A few words upon the medico-moral question of disposition to suicide which some of those suffering under the malady of premature and long-protracted sexual incapacity exhibit. We have never witnessed a case of the disease in which the unhappy condition of the sufferer carried him or her to such lamentable extreme as that of self-destruction, but we have prescribed for a few of those patients the very wretched states of whose feelings appeared bordering upon it. Here is an extract from a letter which we received a few months ago from a patient, the subject of sexual incapacity of some continuance, whose case we had then under our care, and to which we alluded in a note to our last paper:—"I beg you will favour me with a line to-morrow, conveying the result of your deliberation on my case, on the cure of which depends the fate, I may say, both of my *soul* and *body*." And the following is an extract from an instructive work on the diseases of the testicles by Mr. Russell, a surgeon of many years experience in Edinburgh:—"The defect (impotence) rarely goes farther in this country than to prevent individuals from procreating heirs to their estates. I have known very few instances of more calamitous consequences. In one distressing case, of a gentleman of rank and fortune, the effect of premature and excessive indulgences weakened him so much that he lost all enjoyment of life, sank under a degree of despondency which did not admit of consolation, and, though I encouraged him to marry, in full confidence of his recovering his health by prudent management of his constitution, he would not listen to my exhortations, and drowned himself in a fit of despair."—Russell on the Testicles, page 36. In a great many cases of suicide it is certainly difficult, and in some indeed it is impossible, to arrive at a knowledge of the *vera causa* of the sad act, but we have no doubt that the sexual disorders in question sometimes lead to its commission in both sexes, and no human being is ever aware of the dark truth but the unfortunate victims.

Sackville-street, Feb. 7, 1843.

RESEARCHES ON THE BLOOD.

A LATE treatise by MM. Andral, Gavarret, and Delafond, on the condition of the blood in animals in health and in disease, contains the following statements:—The quantity of fibrin normally present in the blood differs greatly in different animals, and holds no strict relation to energy of constitution. The quantity of fibrin is very small in the blood of animals examined within a day after their birth. During the latter period of gestation the fibrin of the blood diminishes to below its normal amount; but immediately after parturition and during the early period of lactation the blood contains its largest proportion of fibrin, and puerperal diseases are apt to occur correlatively with its amount. The blood-globules exist proportionally in greater quantity in the blood of carnivora, and in least quantity in that of herbivorous animals. The greater proportion of globules in the blood is correlative with greater constitutional vigour in individuals of the same species; and improved breeds of sheep are distinguished by an increase in the number of these globules. During gestation, after parturition, and within a day after birth, the globules are diminished or increased correlatively with the fibrin. The serum of the blood is in smallest proportional amount in carnivorous animals, and most abundant in the herbivora. It accrues considerably during the occurrence of pot-belly (*cachexia aqueuse*) in sheep, conjointly with a notable decrease of the fibrin and globules. Dropsy only supervenes as a consequence of an alteration in the blood, when this fluid is divested of a certain quantity of its albumen. The diminution of the blood-globules alone is not sufficient to produce it; hence dropsy is not attendant on chlorosis. But it exists in man when albuminous urine is excreted, and in sheep when the parasite *fasciola hepatica* is found in the liver, a circumstance which is often attended by pot-belly and anemia.—*Gaz. des Hop.*

NOTE IN REPLY TO

DR. GRIFFITH'S REMARKS

ON

THE PITTED TISSUE OF PLANTS, ON MUSCLE, &c.*

By MARTIN BARRY, M.D., F.R.SS.L. & E.

I HAVE to acknowledge the courtesy shown in the remarks of Dr. Griffith, and regret that his opinions differ so widely from my own; but am compelled to say I find nothing in his communication that alters in any particular my views, or that requires more than *general* notice at my hands.

"The appearances observed by Dr. Barry in the blood," which Dr. Griffith thinks were "misinterpreted," I cannot suppose that Dr. Griffith ever *saw*; if I may judge from the description he has given. That they are, however, visible, is proved by the following description given by another, who did see the appearances in question. Whether they have been "misinterpreted" the future may determine.

"Bristol, Aug. 19, 1842.

"Dr. Barry has pointed out to me, among the corpuscles of newt's blood preserved in their own serum, without any re-agent having been applied to them, many which had the form of a flask with a projecting neck, or which might be still better compared to the body of a pair of bellows, with its projecting nozzle. The projecting portion appeared to be a filament, having a much higher refracting power than the general substance of the corpuscle. He also showed me, in a portion of blood to which corrosive sublimate had been added, a corpuscle which was evidently destitute of the ordinary nucleus, and which contained what appeared to be a filament, which presented transverse markings that resembled those of muscular fibrillæ, the interspaces being oblique. The appearance resembled that of Dr. Barry's Fig. 9 β (Phil. Trans., 1842, plate v.), except that there was no trace of nucleus.

(Signed) "W. B. CARPENTER."

My preparations of muscle have been seen by many, to whom I could refer for their opinions regarding them. Among our own countrymen may be mentioned Robert Brown, D.C.L., and Professors Owen and Sharpey, besides the gentlemen from whom I have received the testimonials at foot. To the kindness of Professor Sharpey I am indebted for the beautiful preparation of muscle from the tail of the tadpole mentioned by Dr. Griffith. The following note was sent me by one who had closely examined that preparation:—

"6, Holles-street, Cavendish-square,
"Oct. 13, 1842.

"My dear Sir,—On returning home to-day, after seeing your exquisite preparations of muscular tissue, I was anxious to express my thanks for your kindness and patience in exhibiting the series to me. I went to your house by no means prepared to admit the existence of the spiral fibre, on the contrary, somewhat prepossessed against such a theory; for while I had already made up my mind as to the non-existence of the discs advocated in Mr. Bowman's very ingenious paper in the 'Philosophical Transactions,' I had not been able to bring my belief to the idea of substituting a spiral thread or fibre to account for the peculiar markings on the muscular fasciculi. You have, however, convinced me, for in several instances I was enabled to follow the spiral thread *round* its axis, and to see the continuity of *both* sides of the chain. In one or two instances I observed it drawn out or separated so far as not to leave a doubt of its reality. In the same way I distinctly recognised the double spiral (especially in one preparation, where the two spirals had not an equal obliquity), and I can conceive that the longitudinal lines or fibrillated appearance of the larger fasciculi depends upon the even juxta-position of many minute spirals.

"The reason, probably, I had failed in previously making out this structure resulted from my expectation of seeing this appearance throughout the whole length of a filament; but I now observe how minute is the care necessary to separate parts, and how small often is the portion favourably situated or sufficiently isolated to admit of a distinct view of this curious structure. There are, also, many circumstances connected with the different refrangibility of objects, of great importance in explaining why a spiral fibre should be so much more easily seen in one tissue than another; and thus it is that re-agents are often most usefully applied where different parts of the same object refract the light nearly equally. I *think*, through your help, I have at last settled my belief as to the true character of the markings of muscle, and for which I beg you to accept the thanks of your's faithfully,

(Signed) "JOHN DALRYMPLE."

"To Martin Barry, M.D."

The following, connected with the same subject, was received from Dr. Carpenter, bearing the same date as his testimonial above given:—

"I have this day had the opportunity, through Dr. Barry's kindness, of examining several of his preparations of muscular fibre, especially those from the heart of the turtle and from the shrimp. I have *distinctly seen* single spiral threads continuous with fasciculi; in one or two instances so little elongated as to resemble a corkscrew, in

* Annals of Natural History, No. 68, for Feb. 1843, p. 95.

others drawn out more or less straightly. In several fibrillæ, which had been isolated without disturbance of their structure, I have seen appearances closely corresponding with those represented by Dr. Barry in figs. 52 and 56 of his last paper (Phil. Trans., 1842). I may add, that I have seen these appearances even more distinctly under my own microscope, which is furnished with one of Powell's *latest* 1—16th objectives, than under Dr. Barry's instrument, in which lower powers were used.

(Signed) "WILLIAM B. CARPENTER."

The microscope I use is one of Schiek's acromatics, similar to those employed by Professors Ehrenberg, Schwann, and R. Wagner. On this subject I cannot refer to a higher authority than that of Joseph Jackson Lister, who, after a close examination, describes my deeper object-glasses as "very finely corrected every way."

CHARING-CROSS HOSPITAL.

CLINICAL REMARKS
ON A CASE OF
CONSTIPATION OF THE BOWELS.

By Dr. CHOWNE.

A—— A——, a delicate young girl, of about fifteen years of age, light complexion, and highly nervous temperament, was admitted into the hospital September 28th; she became first the subject of catamenia at about fourteen years of age, which recurred at intervals of three and four months.

About twelve months since she began to have a constipated habit of the body. She states that it began to be troublesome at about the time when the catamenia commenced, and that it has continued to become more so ever since. At first there was a cessation of all alvine excretions for three and four days; frequently seven and eight intervened. At this time she did not have any sickness, but about Christmas last she was confined to her bed, and had during the time constant sickness, throwing up everything she ate. The bowels were much confined, and she constantly required purgative medicine. Before the sickness set in she had not any pain, but during and after the sickness had pain in the chest. She was in the habit of taking croton oil, which sometimes acted and sometimes not. In some instances a fortnight elapsed without any relief from the bowels; and in one instance, as she tells me, and as her mother also affirms, there was an interval of nearly a month. It may be very difficult to determine correctly what was the original cause of the extremely sluggish state of the bowels; there is reason to believe, however, that it was induced, although not wholly, at least partly, by a habit of procrastination, an evil which can scarcely be too strongly deprecated, but which, unhappily, is very prone to exist in young girls, and to lead to difficulties such as that we have had to encounter in the present case, although in a less aggravated degree.

When this patient was admitted she had, in addition to the constipation, a constant sickness after every meal, and, as you have had opportunities of seeing, every time we came to her bed we found in a bason by her side the last meal she had taken, apparently but little changed. There was an appearance of general emaciation and debility in her person, although not at all severe, and the constitution, indeed, appeared not to have suffered so much as might have been expected. The tongue a little, and only a little furred; the pulse and state of the skin natural; renal secretion natural; expression of countenance cheerful; spirits equal; not any sense of pain or of distention in the bowels. She appeared to have been under skilful treatment prior to her admission into the hospital, but whether, in the absence of hospital discipline and regularity, as regards medicine, diet, &c., she had not deprived herself of advantages which she might otherwise have derived, is very doubtful. You will find it a good general practical rule in all chronic cases where the functions have become sluggish and tardy, as in the present case, to endeavour, by a system of habitual regularity, to bring back the natural state of healthy action.

Surmising that the hepatic secretions were defective, and not in a fit state to mix with the chyme from the stomach, I first prescribed for her a common purgative dose of hydrargyri chloridum, followed by a dose of croton oil; this was followed by action of the bowels, in a slight degree. It did not appear, however, that there was either deficiency of bile, or bile in a morbid state, so far as we can judge of such state by the appearances.

As the stomach was still intolerant of both medicine and food, and hoping that stimuli and inducements to action, administered independently of the stomach, might have a purgative effect, instructions were given to the sister of the ward that the patient should daily, and with the strictest regularity, be provided with hot water in a night-chair; that whether the patient had reason to expect an action of the bowels or not, the effort should be made; that in the event of failure an enema of one minim of croton oil and half a pint of barley-water, should be administered, and repeated every two hours, until a fourth had been given, should it be necessary. On the first morning two enemata were administered, and procured copious dejections. The further history of the case may be very briefly stated. She was admitted, as we see by the date of the first prescription, on the 28th or 29th, the enemata were given with good effect; on the 30th the

same plans were adopted, and with success. Upon going to her bedside on this last day I found a return of irritability of stomach, occasionally terminating in the evacuation of its contents, and ordered for her draughts, to be taken every three or four hours, according to the formula in use.—℞ *Sulphate of magnesia*, half a drachm; *carbonate of magnesia*, ten grains; *distilled water*, one ounce. Make a draught. This medicine effectually set at rest the sickness, and as its tendency was also slightly aperient it was continued. The plan with regard to the hot water was regularly persisted in, and after a few mornings the bowels acted well, without any administration of medicine, except the solution of sulphate of magnesia. Up to the present time the regularity of action has been continued, and she will be presented, preparatory to her leaving the hospital, on the next board-day. Although I very often see good effects, such as we have seen in this case, follow the establishment and observance of a regular proceeding calculated to induce a daily habit in the system, yet I cannot look upon this but as an example of more than average success. Although I certainly considered that after correcting the state of the prima via, as it regarded any error of secretion, that might show itself, the plan and treatment enjoined would be successful, I did not look for that success within so short a period. That the improvement took place so soon might, perhaps, be attributed to the youthfulness of the patient; but, unfortunately, we may fairly apprehend, on the other hand, that should the observances which have been enforced during her stay in the hospital be neglected, when she is again left to her own suggestion, it is extremely probable that habits of procrastination and neglect might creep in and bring back the difficulties from which she has been so lately relieved.

BAD EFFECTS OF INJURED MEAT.

DETAIL OF SYMPTOMS.

DEATH OF THREE PERSONS,

AND SEVERE ILLNESS OF FIVE OTHERS.

A small farmer of Wirtemburg, named Ermann, killed a pig, previously in a perfectly healthy condition; and made a kind of sausage of the liver, &c., seasoned with spice, which, having boiled, smoked, and kept afterwards in a cool and damp cellar, he had dressed for his family about eight days afterwards. A neighbour, who with his three apprentices was invited to join the family on this occasion, perceived a sharp taste in the sausage and counselled the rest not to eat of it; only one of his apprentices, however, followed his advice. Ermann and his man-servant ate a considerable quantity; his two sons, one twenty-four and the other eighteen years of age, took somewhat less; two of the neighbour's apprentices ate three or four ounces each; and the wife and female servant of Ermann partook of it, though only to a small extent. Ermann himself ate more on the following day.

Two days afterwards Ermann complained of dryness in the mouth, vertigo, and dazzling sensation in the eyes, which symptoms were soon followed by colic and diarrhœa. The last symptom persisted only until the next day; but it was followed by an almost total loss of sight, singing in the ears, great uneasiness in the extremities, and an increase of the dryness in the mouth and throat. The face was pale, the pupils of the eyes were dilated and immoveable; pronunciation thick and difficult; tongue presenting many red papillæ penetrating through a yellowish-white fur; painful deglutition, apparently due to an obstacle in the œsophagus; little thirst, but a sharpness of appetite singularly contrasting with the dryness of the fauces; no cough, nor difficulty of breathing; no alteration in the intellectual functions.

On the succeeding day the prostration was greater; the eyelids were paralysed, the pupils of the eyes permanently dilated; sight was abolished, the conjunctiva injected; speech very difficult (evidently owing to paralysis of the laryngeal muscles); deglutition impossible; and the pulse feeble and intermittent. But motion remained in the tongue; respiration was still easy; hearing continued perfect; and the intellectual functions were unimpaired.

Next day, after increasing aggravation of the prostration and paralysis, the patient died. Paralysis affected the urinary bladder until death, but had not involved the extremities.

The body was opened thirty-six hours after death. The vessels of the brain and dura mater were found filled with dark-coloured blood. The fauces did not appear inflamed, but only injected here and there. The tonsils were partly filled with purulent matter, and the papillæ of the tongue were much enlarged. The throat and larynx presented neither redness nor other morbid signs; but the lining mucous membrane of the trachea was reddened, and many effused patches were scattered through its submucous tissue. The mucous membrane of the bronchi and their subdivisions was of a dirty-brown tint; the lungs were, in their upper portions, emphysematous, and in some places œdematous; their substance was softened, friable, and gorged with black blood. The left half of the liver was flabby (*flasqué*) and lead-coloured; the right half, on the contrary, preserved its natural appearance outwardly, though on being cut into it gave issue to a flow of blackened blood. Spleen enlarged, and gorged with altered blood (*sang décomposé*). The mucous membrane of the œsophagus was

covered with a whitish fur; that of the stomach presented many effused bloody patches; that of the intestines generally was of a dirty-brown colour.

Ermann's man-servant, twenty-eight years of age, had been taken ill at the same time as his master, with symptoms nearly identical; he died a few hours afterwards, and similar appearances exhibited themselves at his autopsy. The younger son of Ermann was not taken ill until the third day after the meal in question, when he was obliged to leave his school with inability to see the letters in his books, and symptoms similar to those which had first appeared in his father. His malady went through all the stages above noticed, but its march was less rapid: he did not die until the sixth day.

Ermann's eldest son was attacked two days after his brother, but he survived after a month's illness. The paralysis of the eyelids, difficulty of deglutition, and constipation, were the last symptoms to disappear. Towards the end of his illness he had considerable cough and expectoration. The mother, the female-servant, and the neighbour's two apprentices, were also attacked, and obliged to keep their beds during from twenty-five to thirty days.

Dr. Roeser, who reports this circumstance in the "Wurtembergisches Correspondenz-Blatt," remarks that in all the above cases, though in different degrees, the muscles of the eye, tongue, larynx, and pharynx, the large intestine, and the urinary bladder, were paralysed, while the functions of the brain and the movements dependent on the spinal nerves (nearly all the functions of the cerebro-spinal nervous system, therefore) were preserved in all their integrity. He adds that the redness of the mucous membranes that was discovered subsequent to death did not appear to be of an inflammatory nature, but consequent on an altered state of the blood in the vessels, or an exudation of this fluid into the sub-mucous cellular tissue. A portion of the blood of the deceased individuals was sent to Gmelin for analysis; but no account of its peculiarities has, that we know of, been yet made public.—*Gazette des Hôpitaux*, Feb. 4.

INFLAMMATION OF THE MAMMÆ.

To the Editor of THE LANCET.

SIR,—Having experienced the benefit to be derived from pressure in chronic inflammation of the female breast, I beg to call attention to a method adopted in the French hospitals, which, properly managed, at once relieves the patient, and soon cures the disease. The whole breast is encircled with strips of adhesive plaster, until all the parts are included, excepting the nipple, which is left free, as also are any openings for the escape of matter. A starched bandage is then put on over the strips of plaster, and gives support to the parts. I have adopted this treatment in cases where abscesses had been allowed to break of their own accord, where the whole breast was perforated with numerous holes, the skin discoloured, and some hardness present. The effects of this plan are to diminish pain, to induce a new action in the diseased vessels, to give support to the parts, and to prevent deformity. I remain, Sir, your obedient servant,

B. R.

London, Feb. 22, 1843.

MIDWIFERY STATISTICS.

IN the number of THE LANCET for January 7th, ult., page 537, a paper by Mr. Hunter, of Margate, will be found, giving a statistical detail of the chief occurrences in two thousand two hundred and fifty midwifery cases. We may be enabled to amplify or alter some of the deductions to be derived from the above paper by the following statement derived from some clinical remarks lately delivered in Paris, by M. Dubois:—

"Of 484,350 deliveries which have been reported in Germany, France, and England (in what given time or under what other circumstances is not stated), 6330, or about 1 in 76, were multiple births. Of these 6330 births, 6248 were double, 78 triple, and 4 quadruple. The proportion of the double births to the entire number was, therefore, nearly 1 in 78; of the triple, 1 in 6200, and of the quadruple about 1 in 121,000. The proportions, however, seem to be very different in the different countries in question. In Great Britain, the double births were to the whole as 1 to 63, triple births as 1 to 4311, and quadruple births as 1 to 77,613. In Germany, double births were about 1 in 84, triple, 1 in 7,182, and quadruple, 1 in 125,693. In France, double births were in the proportion of about 1 to 92, and triple, 1 to 11,105. The proportionate number of multiple births in Ireland appears to be higher than in Great Britain."

M. Dubois states that in several instances which have come to his knowledge in which the mother has died after a double birth, the two recent *corpora lutea* have been found in the same ovary; and, though this may not invariably occur, he supposes it to be generally the case. In some of these instances the children have been of the same, in others of different sexes, a fact which tends to explode the vulgar notion that the male germs are from the right and the female from the left ovary. In the case of a single fœtus being developed, the corpus luteum seems to be usually on the convex side of the ovary: in multiple developments the corpora lutea have been seen on opposite sides of the ovary. The placentas of double births, seem

on a cursory examination to be so united as to form a single organ of anastomosing vessels. Nevertheless, according to M. Dubois (who has demonstrated the fact by injection), the vessels of the two fœtal systems, in a case of twins, have no anastomotic connection.—*Gazette des Hopitaux*, Jan. 28.

EFFECTS OF ARSENIC UPON SHEEP.

MM. Danger and Flandin, who have performed many experiments with the view of determining the real extent of the poisonous influence manifested by arsenic on wool-covered animals, announce the following as among the latest results obtained by them: A sheep poisoned by a quantity of arsenic inserted under the skin, died in five days. To the last moment it had refused all kinds of food, and the quantity of arsenic in the urine had progressively increased. Another sheep, poisoned by a dose of an ounce of arsenic mixed with a handful of salt (the stomach was probably otherwise empty, though this is not stated), died, as the foregoing, on the fifth day afterwards. It was ill from the moment of having taken the poison, and, like the other sheep, it continued from that time to refuse all nourishment. This last experiment, if performed under the conditions we have above hinted at, would tend strongly to confirm M. Rognetta's theory that arsenious acid becomes harmless to ruminating animals by being involved in a great quantity of food, and its absorption being thereby hindered. One of the most important deductions drawn by Danger and Flandin from their late experiments is, that the public health is not endangered by the sale of mutton from animals to which arsenic had been some time before administered for the cure of disease: for the presence and continuance of arsenic in the system is readily detected; first, by reason that the animals become ill, however small the quantity of arsenic absorbed; and, secondly, that they have never become well again till the last vestiges of the poison have been eliminated by the kidneys and other excretory organs.—*Gaz. des Hop.*

TREATMENT OF INCONTINENCE OF URINE BY THE ERGOT OF RYE.

Dr. Steinbeck, of Brandenburg, administered ergot of rye to two women recently delivered, in whom incontinence of urine had been induced by paralysis of the sphincter of the bladder from pressure during labour. The internal administration of this medicament, coupled with the external use of an embrocation containing tincture of cantharides over the hypogastric region, having been followed by complete success in a few days, Dr. Steinbeck entertained the idea of employing the ergot for similar affections in the other sex. Accordingly, to three old men, in whom sudden chill had brought on incontinence of urine, he prescribed the following mixture:—℞ Ergot of rye, half a drachm; water, six ounces; boil for five or six minutes, and add of belladonna leaves fifteen grains. Suffer the infusion to stand till cool; strain, and add of phosphoric acid (medicinal strength), two drachms, extract of nux vomica, four grains, syrup of manna, one ounce. Mix, and take a tablespoonful every two hours. The patients improved in health, and after a few days the above mixture was superseded by the following:—℞ Extract of belladonna, three grains; extract of nux vomica, four and a half grains; phosphoric acid (medicinal), an ounce. Mix and take fifteen drops three times a day. The treatment in each case was successfully concluded by a course of tonic with stimulant remedies.—*Medicinische Zeitung.*

*** The ergot of rye may certainly have been efficacious in the treatment of the latter cases, but we believe no small share of the success derived to have been due to the strychnia present in the extract of nux vomica.

Dry Feet.—Sir: After having tried many ways to prevent my boots or shoes from becoming damp by walking the streets in wet weather, I find that a thick varnish, made of shell-lac, dissolved in spirits of wine, and spread, with a brush, on the inside of the shoe, all over the sole, and about half an inch on the upper leather, so as completely to cover the seams, will effectually preserve the feet dry, although the sole of the boot or shoe be less than quarter of an inch in thickness. This varnish will not soil or adhere to the stocking, even when the feet become heated from much exercise. I have tried this means for some months, and after experiencing the good results, make this communication to The Lancet in the hope that the knowledge of it may prove of general benefit.
Your obedient servant,
John Norton, late Capt. 34th Regt.
February 27, 1843.

CHARACTER OF THE SALIVA.

M. Budge, in his researches on this fluid, has found that it is invariably alkaline, not only before and directly after taking food, but even after a long abstinence. So necessary would seem to be an alkaline secretion at the commencement of the digestive tube, that in a dog, the parotid, submaxillary, and sublingual glands, of which, on both sides, were extirpated, the mucous membrane of the mouth itself was found to secrete a fluid constantly alkaline. A similar result followed the same experiment on a rabbit.—*L'Experience.*

THE LANCET.

London, Saturday, March 4, 1843.

No town in England contains a more energetic and spirited body of medical men than Nottingham. "Nemo nos impune lacessit," is their motto. The professional world possesses in them an admirable example of union when their personal interests are attacked; and, to their honour be it added, they can, as events have shown, act as promptly on behalf of the large family to which they belong, as for themselves, a section of that family.

What they regard as a new move against their comfort and welfare, has recently been made in the Nottingham Board of Guardians, and they have at once unanimously and consistently offered a determined opposition to the proceeding. The Poor-Law Commissioners, under their "General Order" of March 12th, 1842, proposed to effect a change in the medical arrangements of the union, and the medical gentlemen of the town acknowledge its propriety,—a strong proof, where the corps is *unanimous*, of the wisdom of the decision. The Guardians denounce the change, which relates to the division of the town into four districts, with a surgeon in each to attend the sick-poor of that locality. Not being familiar with the spots concerned, we have not derived from the documents before us a clear notion of the local points under debate; but one thing is plain enough in the matter, namely, that the Guardians itch to bring into the town some youthful strangers—ADVENTURERS we have generally termed these parties, possessing no *esprit du corps*, and willing to undertake anything called *practice*, at any sum called *salary*, who will serve the purpose of a non-medical junto at the expense of the medical practitioners. This most unworthy and heartless mode of coercing humane and respectable men into objectionable schemes has disgusted at last, we hope, even its first authors, the Poor-Law Commissioners, and we trust that they will now act, in relation to it, the discreet part which experience has taught them latterly to adopt when the *vicious* course has offered itself for rejection.

One other thing is plain in the affair. The Guardians seek to maintain the existing most odious of all odious economies,—the economy of medical aid in disease. The present plan, under the incorporation of the union, is *cheaper by one-fifth*, they say, than that which existed before the union. All Nottingham—where misery and sickness are at their height—is to be preserved from disease among its hosts of poor for 350*l.* a-year! The surgeons of the union do contrive, we suppose, to be satisfied with this remuneration, since, by working day and night—the cursed penalty of an adequate subsistence in this country—they add to it the receipts of private practice; but this latter means the Guardians would cut off from their resources. The Committee appointed by the Guardians to report on their own behalf write thus:—

"With so large a number of sick persons under their care, the committee are of opinion that the Guardians ought to have the *entire services* of the medical officers employed by the union, in order to obtain regularity of system as well as uniformity and co-operation of practice. It would most probably be found impracticable to obtain any services equally effective in obtaining their ends, if the union were divided in the manner proposed by the Poor-law Commissioners into four districts, under professional men, who are engaged in general practice, even although the union were to go to double the present amount of expense. When it is considered that the number of cases treated in one year is between six and seven thousand, the committee cannot but think the regularity and uniformity above-mentioned to be a matter of great importance; and considerable economy has been found to arise from an agreement between their medical officers upon simple formulæ of prescription in the more ordinary cases; and the value of the superintendence which the board is able to exercise by receiving a weekly *personal* report, has been greatly enhanced."

Not a word have we to utter against the engagement of the *entire* services of union surgeons. On the contrary, for that prin-

ciple we have ever contended in appointments under Acts of Parliament. But not at a rate of £350 a-year for seven thousand cases—exactly one shilling a case, mock *Guardians* of the poor!

The inkling of the Guardians to obtain tools to promote their ends is developed in the following paragraph. The early years of *privation*, if the adventurer wears through them, may be compensated to him by private practice, when he can kick down the ladder which has borne him towards it:—

"The committee are very much at a loss to discover any objection that has ever been adduced against the system of the Nottingham union, excepting by members of the medical profession, their objections arising from the fact that the medical officers of the union, after their period of engagement has expired, have *occasionally entered into private practice under somewhat superior auspices.*"

They conclude thus:—

"For all these reasons the committee strongly urge the Board of Guardians to communicate their unanimous opinion to the Poor-law Commissioners, requesting that Nottingham may be erased from the schedule of unions in which the 'general orders and instructions as to the medical department are intended to be enforced.' Nay, the committee would even desire to urge upon the Poor-law Commissioners the propriety of re-considering their 'instructional letter,' and of recommending the general adoption of the Nottingham system in every *manufacturing population* of a similar number and extent.

(Signed) "W. J. BUTLER, Chairman.

"Rectory, St. Nicholas',
"Feb. 11, 1843."

A *christian* clergyman, with probably four times the salary of all the surgeons of the seven thousand sick souls! What would the Reverend Mr. BUTLER think of one shilling a *funeral* for the lot? Such guardians really cannot want the poor to be kept alive.

The medical gentlemen of Nottingham met at the Assembly Rooms on the 27th of February, Dr. J. C. WILLIAMS in the chair, and agreed to several resolutions on the subject of the report of the Guardians, with the latter of which we may appropriately terminate these remarks:—

Resolved unanimously,

"That the adoption of a system of medical relief principally on account of the low rate at which medicines are supplied to the poor, is in direct opposition to philanthropy, economy, and justice.

"That this meeting is of opinion that the ratepayers of Nottingham would rather submit the medical treatment of their poor to the resident practitioners in whom they have confidence, than to young gentlemen who, at an inadequate remuneration, undertake the duties for the sake of the experience they may obtain.

"That in drawing the attention of the Poor-law Commissioners to the above resolutions, this meeting disclaims being actuated by any other desire than that justice should be done both to themselves and their poor fellow-townsmen."

THOUGH we may grant a short respite to quacks, intending, however, to lay on the lash again at an early opportunity, we hold no truce with quackery in the abstract. Imposture not only springs up within the pale of our profession—it equally infests society at large, and an ADDISON, or a STEELE, or a JOHNSON, if either could again become a tenant of this breathing world, would find in our day as many vices and follies to castigate as existed in his own time. Among those vices, one of the first attacked by his vigorous pen would be that pestiferous kind of hypocrisy which, under the flimsy mask of care for the poor, indulges a pragmatical and patronising disposition, while it contrives to foster ignorance and enable it to work its dismal way under high authority. One of the literary productions generated by this species of pseudo-philanthropy is to be found in an article in the "Royal Agricultural Society's Journal" for 1842, p. 83, professing to be an essay on "Cottage Economy and Cookery," and compiled from a number of essays which had been sent on a voyage of adventure after the premium offered by the society for the best directions to enable the labouring population to prepare their food in the most economical manner. It is satisfactory to know that none of these essays really obtained the premium, for in perusing the exquisite pasticcio which has been concocted from the rejected essays, what will most readily strike the reader who is at all acquainted with the rural districts, is the utter inapplicability of many of

the benevolent directions laid down to the actual condition of the labourer. Thus, a bullock's-heart, an ox-cheek, a sheep's-head and pluck, are recommended as excellent dishes for the labourer's family—all excellent things *if they can be got*. But the first and second are seldom to be seen in the more remote parts of the country; and even if they were, their price is far above the resources of the agricultural labourer; and the third, although perhaps less rare, is almost equally unattainable, excepting by chance, or in turn, for even in the villages the head and pluck is commonly bespoken of the butcher for weeks before the sheep is killed.

Sir ORACLE, who seems to entertain the notion prevalent among certain classes, that the working man is already quite well enough provided for, sets off by asserting,—"There can be little doubt that bacon, "household-bread, and cheese, washed "down by a draught of strong beer, is a "wholesome and substantial meal for a "working man, and if he can but afford it "he will seldom find a better." He forthwith stultifies himself in the following *morceau*, which we extract entire:—

"Fresh animal food is, however, more nutritious, as bacon, although it goes farthest in family use, and is, therefore, more commonly employed by the poor, yet occasions *much greater perspirations* after hard labour, and thus not only *creates more exhaustion*, but a little change, where it can be had, is both pleasant as well as wholesome. When a cottager has a garden and fattens a pig, he seldom goes to the butcher until all the bacon is consumed; but he would do better by occasionally buying *a pound or two* of meat to eke out the bacon until he can kill another hog."

Genius of LIEBIG! *a pound or two* of fresh meat in the course of a year! for it is notorious that "a cottager who fattens a pig" can rarely afford to kill more than one in the year, which must be eked out with other and less nourishing materials, so as to supply him with food throughout the twelvemonth. But not content with showing that bacon creates "exhaustion," and is far from being the substantial article of food that he had first announced it to be, the author goes on thus:—

"With regard to strong beer, independently of its cost, although its excitement gives momentary strength, yet we know, from those who have tried it, that continual labour will be better performed by a man who during the day drinks only water mixed with some powdered ginger. The powder is extremely cheap; it warms the stomach; and a very small quantity, which can be mixed in a moment with a pint of water, will be found a more invigorating draught in warm weather than the same quantity of common harvest beer."

Having thus thrown aside a really nourishing material of aliment, beer, for a mere stimulant, and already acknowledged one grand element of the "wholesome and substantial meal" to be exhausting and unsubstantial, we find the main stay of the hard-working labourer, who deserves to live *well*, limited to bread and cheese by the economical adviser, who, however, remarks:—"It "is a great error to suppose that butter and "cheese are less expensive than meat!" In a similar strain of sophistry we learn that "the poor are, in marketing, oftentimes "more anxious to obtain a portion of the "best joints than to content themselves with "those of inferior price, although these "generally contain as much, and sometimes "*more* real nourishment." The banyan system for the rising generation is advocated in full force, elaborate directions being given for ox-cheek soup, "a portion of which will "be sufficient for the children, with a piece of "bread, without any of the meat." Bones are to be boiled until a liquor is produced which "tastes *something like* strong broth," then again boiled, "and the broth may be "made the next day into a stew with rice. "Nor is this all, for the bones, if again "boiled for a still longer time, will once "more yield a nourishing broth, which may "be made into pea-soup." Rye-bread is calculated, *on paper*, to be as good, and capable of going as far, as that made of wheat. And in defiance of all experience, and the truths of the doctrine of alimentation as established by chemistry, we are told "that if, instead of putting pie-crust over

"the meat, you cover it with *mashed potatoes*, "and put it either into the oven or bake it "by the side of the fire, it *will answer quite "as well as paste!*" With such kind of rubbish is the article quoted amply garnished, and we have no doubt that the deservedly high authority of the work which it disfigures will cause it to be read, studied, and recommended; and that the unfortunate cottagers who may be unwilling forthwith to adopt the plans pointed out therein for the pleasure of their patrons, will incur a heavy load of odium, and give fresh cause for diatribes against the "lower classes," their "mismanagement" and their "ingratitude."

Away with such wish-wash philanthropy! Medical men, who are the natural advocates of the rights of humanity, and the natural lawgivers respecting the requirements of the animal frame, ought to rise and protest against the public cajolement by statements such as these. The task of indicating the amount of diet that is necessary for the well-being of individuals belongs to them, and they ought not to suffer it to be taken out of their hands by mere amateurs. Let those medical men, moreover, who are attached in their professional capacity to workhouses, or gaols, throughout the empire, ascertain closely that an adequate proportion of food is administered to those for whose health they are responsible; and, directed by the light which organic chemistry in our day affords, let them boldly claim for those whose breath almost hangs upon their fiat, a due amount of daily nourishment, preventing the poor either from being wholly delivered over to the tender mercies of their "guardians," or treated after the directions of unsuccessful aspirants to prizes of Royal Agricultural Societies.

Commentaries on some Doctrines of a Dangerous Tendency in Medicine, and on the General Principles of Safe Practice. By Sir Alex. Crichton, M.D., &c.

A work proceeding from an individual of high standing, who has passed the greater part of a long career in the active pursuits of the profession of medicine,—in early life an hospital physician and a teacher in London,—the contemporary of Drs. Reynolds, Warren (the elder), and Pitcairn, and numbering amongst his pupils the late Dr. Young;—favourably known to the public as the author of "An Inquiry into the Nature and Origin of Mental Derangement;" also holding, for many years, the appointment of physician to the late Emperor Alexander, of Russia,—one who has retired from practice, and from whose bosom is withdrawn (we may presume) in such a production as this, every motive save that of a desire to confer a benefit upon his fellow-creatures, being in the 79th year of his age:—a work from such a source demands attention and respect.

There are three commentaries. The greater part of the first is occupied in demonstrating the erroneous notion entertained by Haller and many of his successors, including Dr. W. Philip, of the possession by the arterial tunics of a muscular power, with the fatal tendency of such an error; but the physiological writers in our own Journal having generally held the same opinion as that of the author, we need not dwell upon it now. One or two statements, however, in this part of the book call for notice.

Upon mathematical data, furnished by the late Dr. Young, the conclusion is arrived at, that a quick pulse is indicative of a slow circulation:—

"The pulse may beat 130 times per minute, and yet the progressive motion of the blood from ventricle to auricle may be slower than in health."—P. 9.

The pulse being both quick and weak, its two most frequent concomitant qualities, the above proposition becomes self-evident, the quantity of fluid to be moved being the same, and provided the admission be made that its motion depends mainly on the action of the heart; so that the rate of pulsation, taken alone, is no index whatever of the progressive motion of the blood; and the only case in which a quick pulse corresponds with the increased celerity of the blood, is where the action of the heart is stronger as well as being more frequent than natural, and accordingly the quantity of blood expelled at each ventricular contraction is either increased, or but little or not at all diminished. In a majority of cases the heart's action being increased in frequency, it is also more feeble, and, as a general rule, it would appear, that the frequency is proportionate to the loss of ventricular power.

In these cases it is that a quick (it should rather have been written frequent) pulse becomes indicative of a slow circulation of blood.

We have enunciated this proposition thus distinctly because we believe that it differs from the reasoning usually adopted by practitioners. A quick pulse is taken generally, if not uniformly, to indicate a rapid circulation. We scarcely need remark, that in certain fevers, for instance, or stages of fever, it makes some *little* difference whether the blood is moving more rapidly or more slowly than in health; and as respects treatment Sir Alexander appeals to the impropriety of the use of remedies which, at the same time that they lessen the frequency of the pulse, diminish also the power of the heart, or the quantity of blood expelled from it at each ventricular contraction, thereby rendering slower an already retarded circulation in diseases where the pulse is quick and weak. We should like to know the objection, if any there be, to the simple proposition—a rapid pulse with diminished power of propulsion in the heart indicates a slow circulation of blood.

A remarkable feature in the work is, that its author, notwithstanding his advanced age, adopts into practical use some of the most important of the doctrines recently promulgated by Liebig; insisting, for instance, on the correct calculation of forces, or the dynamic doctrine, to explain the circulation, and that the blood-vessels, like other organic structures, are endowed with a due share of the vital force. (P. 31.) Contrary to the received doctrines of the day, he also refers sudden fainting, lipothymia, and death from terror, and such causes, not to a direct influence on cerebral and nervous energy, but to the overcoming of the vital force generally; prussic acid, digitalis, and other poisons act upon the same force; and, with Liebig, he refers these agencies to the class of disturbing or chemical agencies.

In the first commentary great stress is also laid upon the important subject of the due regulation of exercise. Violent bodily exercise or great muscular exertion of any kind when the stomach is distended, as after a full meal, frequently lays the foundation of incurable disorganisation of the aorta, pulmonary arteries, or ventricles of the heart. Aortic aneurisms have been traced in almost every instance in Sir Alexander's experience to great muscular efforts, or concussions from falls or blows soon after taking food; but

"Regular exercise, when moderately and gradually increased, may strengthen the structure of the heart, on the same principle that every muscle of the human body is strengthened by frequent and moderate use; that is, more blood than usual being made to circulate through its substance, the process of nutrition is increased, and the fibres of the heart necessarily augment both in size and number."—P. 36.

Exercise is also beneficial by promoting the elimination of chemical irritants from the fluids of the body. Cutaneous, bilious, rheumatic, and gouty affections, frequently disappear under its due regulation, together with a restricted diet, when they resist the influence of medicaments.

The occasion is taken to observe that errors of judgment, in mistaking incipient cardiac affections for hypochondriasis and liver and stomachic complaints, "do not always arise from want of experience, but from a careless and hasty manner, or unpardonable negligence in questioning the patient as to the causes of his indisposition." This we verily believe. Not only in dispensary and out-door hospital practice must *fatal* errors of this kind of necessity occur, from the number of patients *seen* in a limited time, but the same deplorable consequence must unquestionably happen, even in private cases, where the daily gains engross the practitioner's mind, to the total exclusion of the science of his profession.

Typhoid fevers form the subject of the second commentary. The practice in these diseases during the latter part of the last century is first adverted to. It consisted, in the first stage, in abstaining from blood-letting, except under very particular circumstances; in the sparing employment of emetics and purgatives; in a mild but strictly antiphlogistic regimen, blisters, moderate diaphoretics, small doses of ipecacuanha or antimony, cool diluting drinks, the free admission of cool air, and enemata. In the second stage, mild nutritive drinks, wine, Virginian snake-root, and cinchona, were employed, and particular symptoms were combated by opiates and antispasmodics. This was the practice handed down by Sydenham, and adopted by the scholars of Boerhaave and Cullen.

"I have lived long enough," says Sir Alexander, "to have witnessed many strange innovations in the mode of treatment, and I

am sorry to add that, judging by the results, they cannot be counted as improvements."

The practice of exhibiting bark from an early period, that of employing large doses of laudanum in any stage, and the practice of large or repeated bleedings, are the subjects of animadversion, the author illustrating his views by a reference to cases.

One principal object of this essay seems to be the inculcation of the necessity of abstaining from blood-letting in typhoid or adynamic fevers. Thus, the practice of Broussais, Armstrong, and Clutterbuck, is condemned. In doubtful cases the great guide in respect of bleeding is the state of the intellectual faculties, which enables us to judge of the true nature of the fever. "Whenever these are debilitated and confused in the very onset of the malady, typhus is to be suspected," and this rule holds good, even when catarrhal symptoms and pleuritic pains exist.

Visceral inflammations have been erroneously interpreted; even the dothinenteritis of Bretonneau is a consequence and not a cause of the disease. These lesions are all of an atonic kind, and they arise in the course of a fever from vitiated secretions acting on parts in which the smaller arteries and capillary vessels were already in a congested state. Referring to Andral's "Médicine Clinique," it is remarked that, obviously, the treatment by blood-letting failed in the cases there cited, and it was an error in practice to be turned aside from the old and approved method by the knowledge of an anatomical fact which might be so easily misinterpreted.

Liebig's views are professedly adopted in the general doctrine of typhoid fevers. Vital phenomena (vitality) are referred to a distinct force, which may be opposed by other natural forces, chemical and mechanical. In answer to the question, Does contagion act primarily on the fluids or on the solids?

"There are many reasons for thinking that previously to the affection of the brain, and consequently before the rigor and true paroxysm of the fever, the blood and circulating fluids of the human body suffer metamorphoses, and are chemically vitiated by the operation of the remote causes, but more especially by contagion."—P. 109.

And, further, "I confess I am much inclined to think that it is chiefly in the serum of the blood, which is so little if at all vitalised, in which the metamorphosis takes place."—P. 116.

The improvements in practice suggested by Sir Alexander Crichton's experience are the following:—1. A more general employment of emetics in the first stages, upon the principle of evacuating vitiated secretions, particularly when bilious complications exist. This was the practice of Sydenham and Pringle, and after upwards of fifty years acquaintance with the malady, the author considers that owing to the prevalence of the doctrines of Cullen, the employment of emetics has been too much neglected, not only in the commencement, but during the course of the disease. 2. Antimonials have been too much persisted in. This also is attributed to a belief in Cullen's proximate cause. 3. Remedies which decarbonise the blood are indicated. These are not stimulants, nor even tonics in the usual sense, but chemical agents which supply oxygen to the blood, as mineral and vegetable acids. 4. The employment of the warm bath, which requires the very greatest judgment and care in its administration, and if these cannot be given it had better not be proposed as a remedy. 5. The aspersion of cold water, with due attention to the admonitions of Dr. Currie. 6. The employment of belladonna in the condition of brain accompanied with a contracted pupil, as recommended by Dr. Graves, of Dublin.

We cannot conclude our notice of this part of the work without remarking, that the very decided objections of the author to the use of the lancet in all cases of bilious fever (p. 93), are not sanctioned by the experience of practitioners in this country. A moderate bleeding in the early stage relieves the viscera, and gives time for the operation of those evacuant remedies which are so well insisted upon. There is a point at which the aching head and back, the oppressed chest and hypochondrium, the burning heat, are instantly relieved by a very moderate detraction of blood, and the action of the emetic and purgative remedies is rendered safer and more effectual by this preliminary step. Of course, we mean with due attention to the vigour of the patient's constitution, and the force and frequency of the heart's action.

The third and last commentary, on the subject of insanity, is extremely interesting and important, both in a medical and a legal point of view. We can bring before our readers, upon the present occasion, but one of the opinions strenuously contended for by the

author, viz., the non-existence of what is now known, according to the denomination given to it by Dr. Pritchard, as "Moral Insanity." This, according to Sir A. Crichton, includes a number of diseased affections and varieties of moral character, alike dissimilar to each other and to true insanity:—

"The great psychological distinction between passionate emotion of mind in a sane person and the mental affection of a maniac, or the hallucination of a monomaniac, consists in this, that the former ceases instantly and entirely when the object of the passion is accomplished, or, in other words, when the disease is gratified, whereas insanity is not arrested by the commission of a crime."—P. 189.

The author contends that the state of mind in passionate emotion of every variety is totally distinct from that which obtains in insanity, and that the subjects of the two are not, as learned physicians and metaphysicians would have it they are, equally irresponsible agents; he cites cases of "moral insanity" from the writers on the opposite side of the question to illustrate his position, and contends that responsible criminals have escaped the scaffold, while monomaniacs have been sacrificed from defects of medical evidence and incorrect conclusions, involving errors connected with the distinctions here referred to. The whole commentary is well worthy of perusal. We shall probably find some future occasion to return to the subject.

To the commentaries are appended "Extracts relative to the Treatment of Syphilis without Mercury," in the General Hospital of Hamburg. These extracts, also, contain some very interesting matter. The hunger method of cure by reducing the patient to a very low diet, and purging him daily with Epsom salts, is particularly adverted to. The effects of the syphilitic and other poisons which differ from those producing fever, by acting for an unlimited period, are explained upon Liebig's principle of a chemical metamorphosis in the blood. In febrile diseases the chemical change becomes arrested by the nature of fever, which destroys both the desire for food and the process of chylification. The element that is susceptible of the metamorphosis is not supplied to the blood. In syphilis, on the contrary, the food taken daily supplies new elements for the reproduction of the poison, the disease being thus protracted indefinitely. According to this principle the *modus operandi* of the hunger cure is obvious.

Our readers will be struck with the prevailing humoralism of this work.

THE SUPERFLUOUS DISCUSSIONS AT THE MEDICO-CHIRURGICAL SOCIETY

To the Editor of THE LANCET.

SIR,—The observations of "Medico-Chirurgus" in your last number (page 803), respecting the discussions at the Medical and Chirurgical Society, are just. It has for some time past been a subject of deep regret to the well-wishers of the society that the discussions on many occasions have been of the most desultory kind, instead of having reference to the *practical* bearings of the paper before the meeting; some of the speakers frittering away the time by foolish speculative opinions, or giving clinical lectures to gentlemen who were teachers when *they* were pupils! Other speakers talk on all subjects with a fluency and facility which would be quite astounding, were it not pretty well known that they had not long left the lecture-room, and that the speech in the evening was but the second edition of the morning's discourse. Nay, not satisfied with *lecturing* the society, they will every now and then inform the members that they have been reading certain books, and call upon the authors who are present to say whether they have rightly understood them,—as if the society cared one pin whether Dr. A. had capacity to understand Dr. B.'s meaning or not! But the thing really becomes unbearable when Dr. B. tells Dr. A. that he *has* misunderstood him, and Dr. A. in an angry reclamation proceeds to show that Dr. B. does not understand *himself*. These unwearied talkers have not the modesty which generally belongs to twaddlers in another place, and so well pourtrayed by the poet,—

> "Here rills of oily eloquence in soft
> Meanders lubricate the course they take,
> The modest speaker is ashamed and grieved
> To engross a moment's notice, and yet begs,
> Begs a propitious ear for his poor thoughts,
> However trivial all that he conceives.
> Sweet bashfulness! it claims, at least, this praise,
> The dearth of information and good sense
> That it foretells us, always comes to pass."

These observations apply fortunately only to two or three of the *talking* members, and are not levelled at discussion, without which, indeed, the society would be a *caput mortuum*. I would also wish to do justice to the last and present presidents in this matter. Both gentlemen are most friendly to discussion, and during the last president's reign the pages of your Journal will show that the debates were practical, nay, constituted the chief attraction of the society.

If Dr. Williams wants the firmness of his predecessor in recalling the wandering speakers to the point, we ought to forgive him much in recollecting his declaration on taking the chair, that without discussion the society would be as dull and contemptible as the meetings at the College of Physicians. I am not sorry to see many new members, and those junior members, added to the society, nor do I, like your correspondent last week, attribute the twaddling nature of the discussions to that cause alone. Surely the apathetic silence of the senior members may have much influence in the matter!

I have said that discussion is the life and soul of the society; those who have watched its career for several years past will at once acknowledge this. What "a beggarly account of empty benches" presented itself in the old times of the society, when discussion was all but forbidden! Is not the increase in the society's numbers fairly attributable, in part, to the discussions and to their publication? What is the remedy for keeping discussion in its proper channel, and for suppressing the incessant jabberings of the two or three lecturers? In the House of Commons we hear of a general rush to the door when certain members rise to speak, but this, though an effectual, would scarcely be a practical remedy in the Medical and Chirurgical Society. The polite admonitions of the press afford the only cure for the grievance, and if the gentlemen alluded to in this communication fail to perceive to whom the observations apply, they—among all the fellows of the society—will alone be ignorant of the fact. I am, Sir, your very obedient servant,

A LATE MEMBER OF THE COUNCIL.

February, 1843.

P.S. It is worth mention that the most incessant talker deprecates the publication of the debates as a check upon the free expression of opinion. Surely *he* is not anxious to address the society more than "sixteen times in one night."

EDINBURGH INFIRMARY.

To the Editor.—Sir: The uniformly uncourteous and uncivil demeanour of Professor Syme towards some parties in the wards of the Royal Infirmary burst out so offensively against one of his clerks in that institution to-day that I am at once prompted to ask you to notice his conduct, with a view to correct it, for it is alike unbecoming a professor and a gentleman. For instance, at the visit hour, at twelve o'clock to-day, as Mr. Syme was examining a case of burn, he made some remarks on the impropriety of the cotton application to burns, to which the clerk, Dr. Andrews, replied, in the most civil manner, that cotton was applied before the patient was admitted. The professor, as though he had been harbouring a feeling of animosity against this clerk, broke out into a remarkable passion, before students and patients, exclaiming, "Sir, it is no matter whether it was applied before or after admission, and I beg that you will trouble me with no more of your remarks." The students of Edinburgh have for a number of years suffered from the uncourteous behaviour of Mr. Syme, and the sudden ebullitions of his temper, and they one and all would be obliged by your mentioning the undignified demeanour by which Mr. Syme has so long been distinguished, and the feeling which this has naturally created amongst them. I remain, Sir, yours, &c.

AN EDINBURGH STUDENT.

February 23, 1843.

THE BRUMMAGEM APOLOGIES.—FINALE.

MR. WOOD, of Birmingham, and Mr. J. Hodgson, surgeon (his colleague at the hospital), have exchanged letters on this subject, bringing out the following facts and apologies, which we extract *verbatim* from the correspondence, thus conveniently made shorter if not sweeter:—

To Mr. Hodgson.—Feb. 13, 1843.

Sir: Mr. Ledsam states that every important observation in his letter, as to the presumed facts, even to the most objectionable sentences but one, came from your own lips, and that the animus displayed throughout was communicated by infection. I am unable to reconcile the part which you are reported to have taken with your declaration upon the honour of a gentleman at the hospital board, that you had not said anything to Mr. Ledsam which could reflect upon my conduct, and with the positive assurances to the same effect which you have on more than one other occasion given to me. Your name, moreover, was attached to the resolution of the hospital board, which exonerated me from the charges brought forward by Mr. Ledsam. The calumnies upon me were obviously calculated to exalt your own reputation at the expense of a colleague. Your obedient servant,

RICHARD WOOD.

To Mr. Wood.—Feb. 13, 1843.

My dear Sir: I exceedingly regret that the subject which has recently been the occasion of so much pain to yourself and to me should have been revived. It appears scarcely necessary to repeat that I did communicate the facts of the case (and I most deeply lament having done so), not, however, for the purpose of detracting from your professional character, nor in the manner stated in the published letter. I beg to assure you that, throughout the long period that we have been associated at the hospital,

I have never on any occasion done or said anything inconsistent with the esteem which I entertain for your character, and with the good feeling which has always existed between us. I am, my dear Sir, your's very sincerely,

J. Hodgson.

To Mr. Hodgson.—Feb. 16, 1843.

Sir: Your letter has by no means removed the unfavourable impression of your conduct to me; and I shall not again communicate with you unless circumstances should occur which I could not avoid noticing, and I now leave you to settle with Mr. Ledsam the precise portion of your responsibility for the calumnious attack upon your colleague. I am, Sir, your obedient servant,

R. Wood.

To Mr. Wood.—Feb. 17, 1843.

Dear Sir: I am ready to admit that in a strictly professional view I could not justify myself in making the case the subject of conversation with Mr. J. Ledsam. I offer you an apology for so doing, whilst I repeat my denial in the strongest terms of having in that conversation made any statement having for its object to detract from your professional character, or which could be fairly construed to bear such an interpretation. I am, dear Sir, your's very sincerely,

J. Hodgson.

Birmingham, Feb. 28th.—*To the Editor.*—Sir: In consequence of the correspondence between Mr. Wood and Mr. Hodgson, which has recently appeared in two of our local newspapers, it is considered by my friends that I am not called upon to make any further observations on this painful subject, which, you remark, did not originate in The Lancet. I am, Sir, your's, &c.

J. J. Ledsam.

BOLTON UNION.

To the Editor of The Lancet.

Sir,—As you have never been wanting, during your long public career, in the defence of the rights and interests of the medical profession, whenever they were in jeopardy, I trust you will give insertion to the following resolution, unanimously adopted at a general meeting of the medical practitioners of Bolton, held at the Medical Library, on the 16th inst., Mr. Johnson in the chair:—

"The poor-law commissioners having issued an order that the surgeons to pauper unions should be entitled to extra fees for cases of midwifery and surgical operations, and the Bolton board of guardians having, at the late election, evaded this order by exacting a promise from the candidates to renounce their right to their fees,

"Resolved,—That this meeting considers the above evasion of the order in question as unbecoming the board of guardians and unjust to the members of the medical profession, who are thereby made the instrument of their own injury."

I am, Sir, your obedient servant,

Thomas Haworth, Secretary.

Bolton, March 1, 1843.

*** Why do candidates lend themselves to any such promise?

ROYAL COLLEGE OF SURGEONS IN LONDON.

List of gentlemen admitted members on Friday, Feb. 24, 1843:—A. P. Hamilton, C. P. Ring, W. Cantrell, J. T. Kirkman, T. H. Cooper, J. Currie, G. B. Irving, J. C. Robinson, J. I. Hely, E. C. Johnson, J. N. Morse, H. W. Price.

BOOKS RECEIVED.

The Cold-water System; an Essay exhibiting the real merits and most safe employment of this excellent system, &c. By T. J. Graham, M.D., M.R.C.S.L. London: Simpkin and Marshall, 1843. 8vo, pp. 168.

A Treatise on Diet, comprising the Natural History, Properties, Composition, Adulterations, and Uses of the Vegetables, Animals, Fishes, &c., used as Food. By William Davidson, M.D., &c. London: J. Churchill, 1843. Pp. 383.

Dr. M'Cormack's London and Edinburgh Monthly Journal of Medical Science, for March, 1843.

The Illustrated Polytechnic Review, and Weekly Record of Science, the Fine Arts, and Literature, Nos. I to 8, Jan. and Feb., 1843. J. Limbird, Strand. 4to.

NEWS AND CORRESPONDENCE.

Toxicologist.—The circumstances of the case are still under investigation. We do not believe that Mr. Hands would knowingly support a dishonourable transaction.

L. R. P. (New North-road).—Reports of the statements which the fellow has been making have reached us from a variety of quarters. If he be not a little more cautious, and considerably more honest, we shall effectually blow him out of the water. The frauds practised by him at the Hall of the Apothecaries' Company, when he obtained his "license," were highly criminal. His "license" is not worth a rush. Not being a *legally-qualified* apothecary, he might be successfully prosecuted for every medical case that he attends in that character, and is incapable of enforcing the payment of any bill which consists of charges for his attendance in the capacity of an apothecary. If his book-debts of that description amount to

a thousand pounds, his "debtors," by resisting his demands, might render them worth not a five-pound note. The impudence and knavery of the fellow must be exposed and punished. He courts his own ruin. Let us hear of a single additional provocation, and the lash shall be applied to the worthless blockhead to his heart's content.

KIDDERMINSTER.—The report of the inquest held on the body of Ann Bentley, in this town, has been received, but cannot with any advantage be republished in our pages. We never transfer statements of facts in medical science from newspapers (doubting the precision with which they are recorded), and, apart from those related in the *Ten Towns' Messenger*, we have nothing to notice in the report but the charge to the jury. The professed "surgeon" was treating a *medical* case, and therefore may have been acting contrary to law as a practitioner, and if so, the bleeding, by hastening the death but one hour, was a criminal act, and therefore the "death by the visitation of God" must have been a death by the visitation of man. Lastly, the verdicts of coroner's juries should never contain expressions of regret, or opinions, but should simply record facts derived from the evidence, since on the former no proceeding at law can be framed. If the surmise of the coroner, which we give in italics, was just, the phlebotomist, if not a legally-qualified medical practitioner, was guilty of something very like manslaughter. That which hastens or "accelerates" a death, produces and is the cause of the death. We wish this fact was better understood at inquests:—

"The Coroner then proceeded to sum up the case to the jury. Common sense would suggest to him that bleeding was not a fit course to adopt with a patient in such a condition. But it had been stated in evidence that it was a wonder the deceased had lived so long; that she was in such a diseased state that almost anything would have produced death; and though *the fainting caused by the bleeding might have been the proximate cause of her death*, yet, with the knowledge of this fact before them, that her life was as it were hanging on a thread, he hardly thought it would be just to hold Mr. Marston deserving of censure. It was probable that if another medical man had been called in he might have adopted a different course of treatment; but since it had been so clearly stated that almost *any circumstance* would have produced death, it certainly would be unjust to blame Mr. Marston, and consequently the jury could not implicate him in their verdict. The jury having deliberated for a short interval, announced their verdict as follows:—That the deceased died by the visitation of God; but we have to express our regret that Mr. Marston was not more explicit in answering the questions put to him relative to his qualifications to practise as a surgeon or apothecary."

Sir,—As a "union medical officer," I have always taken my district by tender, from year to year, at so much per case, and the board of guardians have this week sent to me a circular, requesting a tender to be again sent in for so much per case. As I understood that such tenders were to be deemed illegal under the commissioners' order of last year, what course, under these circumstances, should I pursue? Do not the poor-law commissioners direct in their order that the boards of guardians should offer a certain sum per year, together with all the fees that are named for operations? U. S.—*** The system of tender is abolished, and we would advise our correspondent to address a letter to the commissioners stating the circumstances related in his questions without a moment's delay.

Sir,—I am surgeon to the union in which I reside, and I have lately taken a partner in my medical practice, and am myself about to become a guardian. Does my accepting the guardianship at all incapacitate me from holding the appointment of union surgeon in conjunction with my partner, or does any objection to that affect the firm? I am, Sir, your obedient servant, A SUBSCRIBER.—*** Our correspondent cannot become a guardian and act as a medical officer at the same time. It would be equally unlawful and irregular for one partner to be a guardian and the other a medical officer. Such a plan would be considered to be too objectionable in principle to be tolerated in practice.

D. S.—It can now be proved, in two cases at least, that poison has been introduced into bodies *after death*. Some parties who have been engaged in these nefarious transactions are in a fair way of walking to Newgate.

If we whipped *Dr. Trigg*, every other such quack might demand a like honour, and he has fellows in thousands equally worthy of the birch.

The "Case of the late Mr. Richard Carlile," by Dr. Thomas Williams, received by us last week, having been converted into a circular, and transmitted to all kinds of periodicals, we must, according to custom, leave it in the disbonourable and disreputable company chosen for it by the author. Mr. Carlile himself would have been ashamed of the fate which has befallen his autopsy.

We shall have room for the letter of *Dr. Ayres* next week.

The communications of *A Surgeon of the Old School*, *Mr. M. D. Thompson*, the *M. D. Extraordinary*, *Scrutator*, *D.*, *Mr. R. T. Lodge*, and *A Retired M. D.*, will be inserted.—*A. B.* is an apothecary, according to law.